Depth and Breadth: A mixed-method investigation of disciplinary approaches to health inequalities and health disparities research

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Abstract

This PhD thesis is an in-depth investigation of the ways academic disciplines shape scientific practice in health equity research, a sub-specialisation of population health. Whilst covering many contexts and aspects of research practice, the thesis is centrally concerned with the way disciplinary training shapes knowledge construction, and the tension this produces in multi-disciplinary research domains.

Specifically, the thesis aimed to: 1) identify the blend of formal disciplinary training present among health inequalities and health disparities researchers; 2) explore how disciplinary training appears to shape epistemological approaches to this area of research, manifest in the design and evaluation of research studies, and 3) assess the impact of these differences on interdisciplinary and collaborative research efforts.

To address the first aim, a bibliometric analysis was undertaken using data from 29,212 publications. The resulting visualisation of the 250 most-connected health equity scholars illustrates the area's general structure, and was used as a sampling frame for semi-structured interviews carried out with 45 researchers, representing 16 disciplines, located in eight countries. To address the second and third aims, interview data were analysed using a Kuhnian theoretical lens, anticipating that disciplinary difference operates via what Kuhn termed disciplinary paradigms, and can be understood to manifest as variability in what Kuhn termed the disciplinary matrix. Specificity and detail is added to this general Kuhnian framework via a broad set of theoretical and conceptual tools drawn from diverse literatures including the sociology of scientific knowledge, sociology of professions, higher education research, linguistics, and science and technology studies.

Results suggest that disciplinary training powerfully and enduringly shapes the choices which feel 'natural' or 'normal' for scientists, with multiple kinds of 'normal' coexisting within the multidisciplinary domain of health equity research. Specifically, variation was apparent in the kind of knowledge researchers seek, formation of research questions, selection and employment of methods, evidentiary standards, conceptualisations of health, use of theory, approaches to complexity, and the broad purpose of scientific research. These features mapped predictably (though imperfectly) onto interviewees' disciplinary training, and aligned quite neatly with the four epistemological styles previously described by Lamont in a study of multidisciplinary grant panels (2009).

Disciplinary training interacts and intersects with other factors, including

national and institutional research foci, funding opportunities, pathways to promotion and publication norms. These factors served to amplify (and, in some cases, were amplified by) epistemological styles. In particular, epidemiological norms and standards emerged as especially important in population health, for researchers working inside and outside epidemiology.

While a strong desire to obtain ‘useful’ knowledge cut across disciplines, this was not a unifying epistemological feature as ideas about what is useful varied across disciplinary groups. Interdisciplinary collaboration was presented as important, and desirable, but as being complicated by the challenge of communicating with members of other disciplines. The integration of disciplinary perspectives appears to depend on researchers who speak multiple disciplinary ‘languages’, however this was not felt to be highly valued within (mono-disciplinary) evaluative contexts, such as peer review and academic promotion. Repeatedly, diversity in terminology and meaning appeared to be grounded within (and to reflect) diverse epistemological commitments and disciplinary paradigms.

Collectively, findings demonstrate the presence of diverse ways of knowing within health equity research, and the role of disciplinary training in establishing and sustaining these varied epistemological styles. Differences between disciplines are not incidental, but reflect diversity in fundamental epistemological commitments and diverse strategies for selecting and accessing objects pre-supposed to be valuable, and relevant to health. Disciplines have been framed in some literatures as monolithic or obsolete, however, results of this PhD project support a rehabilitation of the Kuhnian view of disciplines: As well as being social entities, academic disciplines are cognitive enterprises which powerfully shape 21st century research about health in important, interconnected ways.

Lay Summary

This thesis is about the way academics from different disciplines conduct, design, and make sense of health equity research (studies of differences in health between social groups). It has previously been suggested that researchers from different disciplines approach this area in different ways, but research has not yet focused on the impact of disciplinary training on the study of health equity.

The thesis had three aims, to a) identify the blend of formal disciplinary training present among health equity researchers, enabling analysis of the distribution of disciplines across this area; b) explore diversity in the ways researchers seek and generate knowledge about health equity, including considering how such diversity corresponds with disciplinary background, how such difference is reflected in the design and evaluation of research studies, and intersects with other factors (such as geographical location, and funding opportunities); and c) assess the impact of disciplinary differences on interdisciplinary and collaborative research efforts.

To address the first aim, an analysis of published research was undertaken, using data from 29,212 publications. The resulting visualisation of the 250 'most connected' health equity scholars illustrates the area's general structure, and was used as a sampling frame for a set of semi-structured interviews carried out with 45 researchers, representing 16 disciplines, located in eight countries. To address the second and third aims, interview data were analysed via the theory of Thomas Kuhn theoretical lens, anticipating variability in the elements of what Kuhn termed the 'disciplinary matrix'.

The thesis finds that disciplinary training appears to shape the design, conduct and evaluation of research about health equity in interconnected ways. Disciplinary training seems to influence research via shaping research questions, research methods, and the kind of evidence researchers find persuasive. Analysis of interview data revealed varied conceptualisations of health, diverse forms of knowledge, use of theory, view of methods, and approaches to complexity which mapped predictably (though imperfectly) onto interviewees' disciplinary training.

The interaction and intersection of disciplinary training with other factors (e.g. national and institutional research foci, funding opportunities, pathways to promotion, publication norms) was also carefully considered. These factors were found to amplify (and, in some cases, were amplified by) particular ways of approaching research. Norms and standards within

Epidemiology emerged as especially important, both inside and outside that discipline.

While a strong desire to obtain useful knowledge cut across disciplines, this was not a unifying feature, as ideas about what is useful varied across disciplinary groups. Similarly, the concept of 'statistical significance' was broadly viewed as lacking meaning, but no consensus was evident regarding what kind of significance should replace it ('social'/'clinical'/'practical'). Findings suggest that resolution of ongoing debate within health equity regarding the meaning of terms such 'robust evidence' is unlikely, as diversity in meaning appears to be grounded within (and to reflect) diverse epistemological commitments and disciplinary paradigms.

Interdisciplinary collaboration was presented as important, and desirable, but also as being complicated by the challenge of communicating with members of other disciplines. While the integration of disciplinary perspectives appears to depend on researchers who speak multiple disciplinary 'languages', this was not felt to be highly valued within (mono-disciplinary) evaluative contexts, such as peer review and applications for promotion.

Collectively, findings lend empirical support and detail to general claims that disciplinary paradigms inform different approaches to population health, and reveal the ways such differences can manifest in the design, conduct and evaluation of research projects. Disciplines have been framed in some literatures as obsolete, however, results of this PhD project support a rehabilitation of the Kuhnian view of disciplines: As well as being real social entities, academic disciplines are real cognitive enterprises which shape 21st century research about health in important, interconnected ways.

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Table of Contents

List of Figures & Tables

Abbreviations

EAP: English for Academic Purposes

ECR: Early-Career Researcher

HIDR: Health Inequalities and Disparities Research

SDoH: Social Determinants of Health

SQL: Structured Query Language

SSK: Sociology of Scientific Knowledge

STS: Science and Technology Studies

WHO: World Health Organisation

Prologue: My Interest in the Topic

My undergraduate training, undertaken in Australia, was a ‘double’ degree in Biomedical Science and Economics, a program designed to produce Health Economists fluent in the biomedical languages of anatomy, physiology and biochemistry. However, simultaneously studying health from these two angles proved extremely challenging, and over 90% of enrolees had dropped one degree by the fourth year.

I found the experience interesting, because the two streams of lectures seemed to be taking place in different worlds. In biophysics I learned that empirical observation has the last word; if objects stopped falling towards the earth then the theory of gravity will be revised. In economics, the relationship between theory and observation felt different, and challenges to the discipline’s fundamental ideas seemed less welcome. In epidemiology, individuals were members of populations whose health was determined by a combination of known ‘risk-factors’ and random chance. In Health Economics, individuals were self-optimising agents in control of their own health, who elected to invest (or not) in health, according to a set of preferences, subject to budget constraints. These contrasts interested me, and seemed important, even crucial, for the progress of research about population health.

I attended an international conference on the topic of health inequalities during my undergraduate training. Research about social inequalities in health seemed to involve a particularly rich blend of disciplines, with epidemiologists, medical doctors, sociologists, psychologists, geographers and economists answering the same basic question; why does health seem to vary with social variables like income, education, and postcode? And how can we take action to level the distribution of health across these groups? Being young and rather self-possessed, I thought it would be helpful to highlight the obvious (to me) interdisciplinary tensions at play. I assumed others would be interested in and keen to discuss the way views of population health varied across disciplines. To try and stimulate dialogue, I asked questions of presenters at the event, such as:

> "Someone with economic training would probably reach the opposite conclusion you have reached, based on these data, as the behaviour you're describing may be viewed as rational according to economic theory. Given this, how is the change in policy you have suggested is necessary going to be achieved?"

Or, to an economist:

> "Researchers without economic training would probably not agree that a reduction in rent should be treated as 'income' in this analysis, because it is not income, it is an avoided cost. How would you justify this choice to researchers outside economics?"

The awkward silences and sideways looks I received were telling. A researcher I had never met took me aside, saying something like "I understand what you're trying to achieve here, and it is important, but you're only going to frustrate people and cause people to dislike you."

Ten years have now passed, and, professionally, I settled into what I perceived as the neutral territory of statistics after completing my dual undergraduate degrees. I have a clearer sense of why my questions were not enthusiastically tackled by conference presenters, as I have myself experienced the strong pull to work within rather than between academic disciplines. But, having worked as a statistician in departments of epidemiology and economics, my sense that disciplinary differences are important has only increased. I have been watching and waiting for somebody to publish a detailed analysis of how the epistemologies underpinning the various disciplines which study population health (especially health equity) fit together, or don't. Waiting for someone to sketch the disciplinary landscape of health inequalities research, to ask whether long-standing tensions and debates are supported by the epistemic pillars of dominant disciplines. Waiting for someone to explain how genuinely interdisciplinary research might be approached in the presence of apparent division.

But no one did, so here is my best effort. Thank you for reading my thesis.

Chapter 1: Introduction & Aims

> Disciplinary cultures are a readily acknowledged condition of the profession […] this is not to say that we understand the implications of disciplinary differences. Ruscio, 1987:p33.

1.1 Academic Disciplines & Population Health

Population health[1] is a multidisciplinary research domain, including researchers from the natural, biomedical and social sciences. Progress in understanding the health of populations might therefore depend on cooperation and communication between members of different disciplines. However, even as the study of interdisciplinarity expands apace (LERU, 2017; Bammer, 2013), this has not developed atop an established set of theories describing what disciplines are (Krishnan, 2009), or systematic empirical accounts detailing how (or even whether) disciplines condition the content of 21st century science. Disciplines are an obvious feature of the academic landscape, but their specific study has been limited, perhaps because (as the above quotation from Ruscio suggests) they are at once obvious and invisible. While disciplines take grand institutional forms, their influence on specific scientific outputs is challenging to tease out and clearly describe.

When disciplinary difference is encountered in population health research, it is often in the form of general reference to diversity in disciplinary “norms” (Stuckler et al., 2015) or “cultures” (Pilkington et al., 2016). These vague references reflect how little evidence is available describing the substance of disciplinary difference in population health. In addition, it is not clear which disciplines participate, and in what ratio. Epidemiology is clearly a central and dominant discipline (Reubi, 2017;

1 There is some debate about what "population health" means, and whether it is a synonym for "public health" (Kindig, 2007). I use the term here with the meaning explicated by Kindig and Stoddart (2003: p.381), who define population health as including research concerned with “the health outcomes of a group of individuals, including the distribution of such outcomes within the group”.

Pearce, 1996), but population health includes representatives from distant (e.g., engineering) as well as neighbouring (e.g., medicine) domains, and epidemiology itself may be splintering into sub-specialisations (Pearce, 1996). Such diversity likely has consequences for knowledge construction, evaluation, and collaboration. Population health may therefore represent a setting where key similarities and differences between disciplines play out in a manner which can be observed and analysed, and also a domain where insight gained from such analysis would be of significant practical benefit.

In this thesis, this possibility is explored within the specific setting of health inequalities and health disparities research, a multi-disciplinary field within population health. The thesis provides a detailed account of the widely acknowledged but poorly-described differences between key disciplines in this area (and the forces which appear to sustain and moderate them), and explores the challenges researchers face producing multi- and interdisciplinary research in the context of these differences.

In this chapter I introduce health inequalities and disparities research, and outline the heterogeneous (disciplinary) approaches apparent in the literature. I outline key debates and suggested underlying disciplinary tensions, and summarise existing empirical studies of this research area. The aims and outline of the thesis are presented in the concluding section.

1.2 Health inequalities and health disparities research (HIDR)

Health inequalities research and health disparities research (hereafter HIDR or 'health equity research') aim to understand, explain and reduce the unequal distribution of health across groups defined by socio-demographic factors such as education level, income and ethnicity. Differences in health outcomes associated with socioeconomic position have been extensively documented, and those with fewest resources (variously defined) tend to have the poorest health (Graham, 2009). Such inequalities have been documented in all developed nations (Crombie et al., 2004). For example, recent data suggest life expectancy and all-cause mortality improvement in the UK has stalled since the early 2010s, with increased mortality among the most deprived populations (Walsh et al., 2020).

Analysis of these observed differences in health has led researchers to study the social, economic and environmental factors which influence health throughout a person's life-course, collectively referred to as the Social Determinants of Health (SDoH). The SDoH include factors such as, income, education, stress, early-life experiences, social exclusion, employment, social support, available nutrition, and access to transport.

1.2.1 Upstream, Downstream

Health equity scholarship is frequently guided by a conceptual model whereby inequity in health is understood to be caused by underlying inequality in the distribution of the SDoH (Carey & Crammond, 2015). Within this model, SDoH are termed 'upstream' determinants, considered to be outside individuals' control. These include income, education, early-life experiences and social class. The 'downstream' determinants (such as diet, smoking, and physical exercise) have clearer physiological impacts, but are also understood to be connected to upstream determinants. An influential, formal model describing the relationship between upstream and downstream determinants is the "rainbow" model, pictured below (Figure 1). In this model, macro and upstream determinants influence the downstream determinants of health, which in turn impact human physiology.

Figure 1 - The rainbow model of health. Source: Dahlgren & Whitehead 1991

Quantitative analyses describing strong associations between upstream determinants and health outcomes accumulated throughout the 1980s and 1990s (e.g. Lynch et al., 1998).

Elucidating the causal pathways by which ‘exposures’ (e.g. drugs, treatments, risk-factors) impact health is the central aim of epidemiology (Susser, 1985:p.171), and randomised studies are the gold-standard for determining whether such relationships are causal. However, individuals cannot be randomised into social classes, childhoods, or cultures. The apex of the ‘evidence pyramid’ (Petticrew & Roberts, 2003) is therefore out of reach, complicating the evaluation of scientific claims about the SDoH.

Early studies presenting upstream factors as independent, causal determinants of cardiovascular disease and mortality (Barker & Osmand, 1986; Marmot et al., 1984; Kaplan & Salonen, 1990) were criticised for inconsistent estimates, and for the handling of confounding factors such as social and material disadvantage. A review of 19 such studies (Elford et al., 1991) concluded that results failed to support the existence of a causal connection between early-life exposures and adult cardiovascular outcomes, and warned that “the very nature of the hypothesis represents methodological problems that may prove to be insurmountable” (p.833), reflecting the uneasy fit of questions about the SDoH within a classical (i.e., clinical) epidemiological framework. Health equity researchers countered that causal pathways are likely to be complex, varied and multifactorial, and that a lack of definitive understanding along the causal chain should not lead to the conclusion that upstream factors are unimportant, or less important than ‘downstream’ determinants (e.g., House, 1996).

Today it is more widely accepted that some determinants of health cannot be feasibly or ethically randomised, and Social Epidemiology has emerged as an epidemiological specialisation to tackle “the study of how the social world influences–and in many cases defines–the fundamental determinants of health” (Berkman et al., 2014). However, debate about the presence and direction of 'causal' links between upstream and downstream determinants persists in HIDR (Mackenbach, 2020; Mackenbach & De Jong, 2018;

Lundberg, 2020), and the early scepticism about upstream determinants suggest that claims about population health are not evaluated within a vacuum, but stand or fall based on alignment with dominant epistemological frameworks, a possibility which this thesis explores.

1.2.2 Terminology

Diverse terminology is employed within HIDR, and was evident in the 1970s and 1980s, as researchers described observed differences in rates of illness between groups in the post-war UK and US. Disentangling the precise definitions of key overlapping terms is not straightforward, however Table 1, below, provides a guide.

Term	Definition	Often Used in	Often shorthand for…
Health Inequalities / Inequalities in Health	"Systematic differences in health between different socioeconomic groups within a society. As they are socially produced, they are potentially avoidable and are widely considered unacceptable in a civilised society" (Whitehead, 2007: p.473)	UK, Europe	Differences in health outcomes between better- and worse-off groups defined by the social hierarchy, e.g. Class, educational attainment, income, etc
Health Inequity	"Refers to those inequalities in health that are deemed to be unfair or stemming from some form of injustice." (Kawachi et al*,* 2002: p.647)	Worldwide	
Health Disparities	Generally, disparity means 'difference', however its usage in public health research refers to the subset of differences in health outcomes which are perceived as inequitable, as per Whitehead's definition above (Braveman, 2006)	USA	Inequitable ethnic or racial differences in health

Table 1 Health inequalities, health disparities and health Inequity. Definitions and common usage.

The terms in Table 1 each refer to mature and active academic specialisations, wherein authors from various academic disciplines and clinical specialties report their findings in top medical, epidemiological, and public health journals (Bouchard et al., 2015). Understanding the extent of connectivity between these communities, and the origins and sustained use of so many similar terms is one topic of focus in this thesis. Rather than selecting a single term to refer to the field, I jointly describe ‘Health Inequalities’ ‘Health Disparities’ and ‘Health Inequity’ research under the umbrella term ‘health equity research’.

In referring to the kinds of differences in health which HIDR aims to describe and understand, I refer to ‘health inequalities’, as this is the term which dominates in the UK. However, in using this term I include the kinds of differences which health disparities researchers study and describe.

1.3 Heterogeneity in HIDR

Despite many decades of research activity, health equity is not improving at the pace researchers wish (Mackenbach, 2020; Garthwaite et al., 2016). The perceived failure of health equity scholarship to produce policy change and/or demonstrable improvement in health inequalities has generated critical and reflexive discussion (Bambra et al., 2011; Lynch, 2017), revealing tensions which appear to reflect disciplinary difference in approaches to research. Chief among these is tension between explanations for health inequalities framed in terms of factors operating at the level of the individual, and explanations which focus on structural or group-level attributes. The various positions are briefly sketched in the following paragraphs, including supporting theory, where relevant.

1.3.1 Individualist approaches

> *“From the atomistic or individualist point of view, the individual is the source of all new characteristics or transformations, so that the group or population is nothing but the additive outcome of such initiative by the individual.”*
> (Piaget, 1967: p393)

Individualist approaches focus on characteristics belonging to individuals, and conceptualise individual outcomes and/or behaviour as the result of factors operating on, and activating processes within individuals. Groups or populations might be studied, but these are conceived as collections of individuals. Within HIDR, this includes analytic approaches which locate the causes of health inequity within the behaviour of individuals (the 'behavioural thesis'), and approaches which frame health as the result of rational choices made by individuals, such as classical microeconomic approaches.

Historically, a third strand of research also fits under this category. The ‘selection thesis’ proposed that poor health is a determinant of an individual’s position in society, contrasting with the view that material deprivation leads to poor health, and not the other way around. While there is widespread acknowledgement that poor health does contribute to material deprivation (Pantazis et al., 2006), the empirical focus within HIDR has tended to fall upon the ways in which income and social circumstance shape health. Reflecting this, the idea that social position is determined by health status is referred to as ‘reverse causation’ within HIDR (e.g. Marmot, 2017). The selection thesis contains echoes of the eugenics movement of the Victorian era, wherein civic worth was considered a hereditary trait, and “poverty, crime and stupidity arose from the hereditary weakness of the poor, the criminal and the mentally-defective” (MacKenzie, 1981:p.33). Due to desire to maintain intellectual distance from eugenic ideas and essentialist assumptions about those experiencing disadvantage, there has been some reluctance within HIDR to fully explore the possibility that health influences position within the social hierarchy (Marmot, 2017).

Interest in genetic explanations for health inequalities has re-emerged, especially the field of epigenetics which studies how genetic expression may be altered during the life-course, and also whether and how these alterations are transmitted to subsequent generations (Relton & Smith, 2010). Excitement about the anticipated contribution of epigenetic research, and a desire to understand how the SDoH get “into the body” (Williams &

Mohammed, 2008 : p.39) occur in the context of a strong desire to maintain political pressure for ‘upstream’ action on health inequalities and disparities, creating concern about potentially mixed messages to policy makers. This concern was expressed by prominent HIDR researcher Mackenbach (2006), who offered the tentative conclusion that genetic determinants “may play a part, albeit a rather distal and modest role, in the explanation of health inequalities”(*ibid,* p.271) alongside explicitly articulated anxiety about resurrecting eugenic ideas, and diverting attention from social determinants. In addition, studies of migrant populations have demonstrated that immigrants quickly achieve health profiles more similar to their country of residence than country of origin (Guendelman & Abrams, 1995), cutting against genetic explanations for health inequalities. Epidemiological methods have been positioned as being poorly suited to the nuanced exploration of contextual and contingent cultural factors (Nazroo, 1998).

1.3.2 Structuralist Approaches

> “From the holist or totalist point of view, everything happens at the population level, so that the individual is nothing but a passive reflection or, at best, a partial one, of processes which are quite independent of him [sic] and belong to quite a different genetic scale” (Piaget, 1967: p.393)

From the structuralist perspective, health inequalities arise due to processes largely independent of individuals. Key processes are emergent, arising from *relations,* therefore relationships between individuals, institutions and other social entities are a focus of study. Two prominent strategies for accessing these relations within HIDR have epistemological origins within the structuralist thought-style; the importance of place, and the value of studying lived-experience.

The importance of place

Places were recognised as important by the earliest researchers of health equity, for example, Chadwick’s focus on the concentration of disease within neighbourhoods, or homes (Chadwick, 1842). However this interest was not sustained throughout the 20th century (Mechanic, 1993). Beginning in the 1990's, renewed interest in contextual effects and regional variations in health emerged, though not without debate regarding the magnitude of these effects (Macintyre et al*,* 2002; McCulloch, 2001, 2001a) and quality of empirical studies (Pearce, 2013). More recent efforts focus on moving beyond this *context versus composition* debate, to focus on the interactions between people and places. For example, studies of *neighbourhoods and health* explore the ways neighbourhoods mediate between structural drivers (social, environmental, geographic and economic) and health equity (Pearce, 2013). Within this research area, place is conceived as a way to expose and understand sociological phenomena, and also as permitting a theoretically sophisticated approach to social concepts such as class and social capital, via study of the ways in which these forces "work themselves through into the dynamics of everyday life." (Popay et al, 1998: p.635).

Lived experience

Quantitative representations of health inequity are viewed as 'hollow' by some researchers who approach HIDR from a structural perspective (Elliot *et al.,* 2016). Felt to be missing is adequate representation of "experiences of hardship, subjugation and alienation which cannot easily be captured and weighed via ratios, metrics and indices" (Trahair, 2014: p.6). Articulations of such experiences are considered analytically valuable from a structural perspective, because the meanings which individuals attach to experiences are understood to shape social action (Popay et al., 1998) which in turn influences health and wellbeing. Within this "new enthusiasm for the fine grain" in social HIDR (Williams, 2003 :p.141), expressions of meaning in *narrative form* are considered especially valuable (Baum 1995, Popay et al, 1998), as the narrative is "an ontological condition of social life" (Somers, 1994: p.614) viewed as a format within which the relationship between social structures and individual agency is revealed. *Identity* is an important concept and focus of study within this framework. Somers (1994) argues that individuals develop their identities by locating themselves within a ‘repertoire of emplotted stories’ (*ibid*: p.614). Therefore, the collection and analysis of stories about health inequalities in narrative form is viewed as providing a richer understanding of mechanisms at play than quantitative approaches.

1.3.3 Epidemiological Approaches

Historically, Epidemiology is a scientific offshoot of medicine (Susser, 1985). Despite ecological origins (as environments and community characteristics were identified as principal drivers of infectious disease through the 19th and early 20th-century) over time, emphasis in epidemiology has shifted to focus on individual, biological determinants of health (Diez-Roux, 1998; Pearce, 1996). Authors approaching HIDR from the structuralist perspective have criticised epidemiology for what they perceive as an overt individualistic epistemology. This criticism may seem odd, as epidemiology is the study of populations (Last, 2001). However, epidemiological populations are typically conceived as simple aggregates of individuals, with analytical emphasis on individualised measures of difference (Shim, 2002; Diez-Roux, 1998; Pearce, 1996; Krieger, 1994). Social variables are routinely investigated, but are typically individualised, operationalised as if analogous to characteristics such as age or gender (McMichael, 1999). Nevertheless, it is *possible* to investigate extra-individual determinants via epidemiological methods (this is the goal of social epidemiology, a growing sub-specialisation within epidemiology discussed in the next chapter). As norms governing the ‘proper’ use of scientific methods are socially determined and sustained (Barnes, 1982), disentangling the epistemology of methods from epistemological commitments held by researchers requires careful analysis. Such analysis is one goal of this thesis.

1.3.4 Intermediate positions

Tension regarding the importance of individual and social determinants within HIDR includes ground well-trod as part of the structure and agency debate (Frolich & Potvin, 2010), including perspectives attempting to balance the individual and the social, and positions from which one is understood to dominate. This summary would be incomplete without acknowledgement of the intermediate position, perhaps more common in practice than either extreme, which allows for both individually- and socially-located determinants of health. However, rather than resolving the structure-agency question, this raises the question of how (or whether) researchers manage the intermingling of determinants operating at different levels (individual, environmental, social) in designing research projects. Also unclear is the extent to which the methods employed to manage these multiple 'levels' reflect researchers' understandings and beliefs about the causes of health equity.

1.4 Tension within HIDR

In the 1990s, researchers favouring structural, sociological explanations for health inequalities were increasingly frustrated by perceived research focus on individual-level factors, characterised by some (Kelly & Charlton, 1992; Stott & Kinnersley et al, 1994) as 'victim blaming'. Several researchers directed criticism toward methodologies used to generate individual-level explanations, especially the collection of methods known as risk-factor epidemiology (Williams, 2003).

Writing from a structural, ecological perspective, Nancy Krieger (1994) views the biomedical individualism underpinning epidemiology as problematic, leading to a focus on behavioural explanations at the expense of wider forces shaping the 'web' of individual risk-factors. Medical sociologist Leonard Syme similarly rejected individual, behavioural explanations for health inequalities in his article "To Prevent Disease" (Syme, 1996). British sociologist Jenny Popay and colleagues (1998) expressed disappointment that "by far the most numerous of the studies directed at understanding inequalities in health have been focused on exploring the role of [individual] risk factors" (p622), arguing that epidemiological frameworks in general, and risk-factor epidemiology in particular "fail to capture the complexity of causal explanation in the health inequalities field” due to neglect of social structures (Popay et al, 1998: p.627).

Over 20 years after the publication of “To Prevent Disease”, Syme published a paper with a very similar title, articulating an identical set of frustrations with the dominant model of population health (Syme, 2007). Tension between individual and social accounts of health equity apparent in the 1990’s seem to have persisted throughout intervening decades. In a recent study, one academic interviewee referred to the expectation that research about inequalities should be conducted via quantitative, epidemiological methods as an intellectual “straightjacket” (Garthwaite et al., 2016: p.466).

A schism within HIDR is therefore apparent, and might be crudely sketched as consisting of researchers with social science training on one side and ‘risk factor epidemiologists’ on the other. Understanding the epistemological basis of this tension and its impact on interdisciplinary collaboration is one goal of this thesis.

Positioning health economics within this dynamic is not straightforward, as economics is a social science but has its own particular epistemology, and is overwhelmingly dominated by quantitative methods, to be explored further in Section 1.6, following a brief review of empirical studies concerned directly with HIDR.

1.5 Empirical studies of HIDR

Smith and Eltanani (2014) describe understanding division among health inequalities researchers as being “crucial to understanding the difficulties of trying to achieve evidence-informed policy” to combat inequity in health (Smith & Eltanani, 2014: p.567). To my knowledge, all existing studies of HIDR have occurred within the UK, and have focused on the relationship between HIDR and the UK policy context (Bartley, 1992; Smith, 2008; Smith, 2013; Smith & Eltanani, 2014; Garthwaite et al, 2016). These studies have variously identified division within academic HIDR relating to (1) political-ideological outlook/position; (2) methodological approach; and (3) medical credentials/status.

Bartley (1992) focused on debates about the impact of unemployment on health (an important strand of early HIDR within the UK) and analysed the academic and policy contexts within which the first evidence that employment status impacts health was produced. Bartley identified three key (academic) communities; “Statisticians working on public health issues... Economists involved in work on either health or labour market issues [and] medical sociologists” (ibid: p.217) and concluded that friction between these groups established a series of boundary disputes inside government and academia, as the epistemology and professional ideologies of each group influenced their position in relation to ‘the facts’ about unemployment and health. Medical doctors emerged as a fourth important group, with policy influence not accessible to other disciplines. Bartley concluded that it appears difficult for any non-medical discipline to gain 'secure status' in medical research (p.130), especially where the problem of health inequity is framed (politically) as relating to the supply of medical care, rather than to upstream (social) policy.

Bartley presents economists (documented as actively establishing and nurturing political alliances and providing rapid, conclusive ‘answers’ to stakeholders) as the “natural competitor” of the epidemiologist and biostatistician (p.176), for whom cautious interpretation, scientific accuracy and objectivity were the highest priority. Bartley argues that individualist, behavioural accounts of poor health among the unemployed gained influence as a result of this entrepreneurial activity by health economists. Where she anticipated that her study would validate anecdotal claims that ‘the media’ were the originators of individualist accounts, her analysis led her to conclude that:

> “It was the entrepreneurial activities of some economists, their established alliances and new enrolments which they sought, which produced an account of the poor health of the unemployed as a product of individual characteristics” (p.222)

Bartley documented a clear split, along disciplinary lines, on the question of whether poor health outcomes among the unemployed are explained by the causal, upstream impact of unemployment on health (as argued by epidemiologists and biostatisticians) or by the selection of unhealthy people *into* unemployment, or unemployed people consuming more health-harming products such as tobacco and alcohol (as was argued by some economists).

This debate never fully resolved, with both individual and structural perspectives persisting. As many actors from both sides of the debate moved into HIDR (e.g. Alex Scott-Samuel, Adrian Sinfield, Adam Wagstaff, Jennie Popay) it is possible that this dynamic persists. However, no study to date has attempted to articulate, investigate or understand the precise epistemological tensions between economics, sociology and epidemiology/biostatistics in present-day HIDR, a gap this thesis seeks to address.

Smith (2008) analysed policy documents and interviewed 61 individuals (25 academics, 36 non-academics) to investigate the extent to which evidence about health inequalities had travelled into policy in England and Scotland, in the decade following the 1997 election of the Labour government.

Smith found that notions of credibility appeared to vary in important ways across academic groups, and that ‘perceptions about the political and ideological commitments of researchers’ (p.189) were central to interviewees’ sense of who had credibility and who was an ‘amateur’. Smith’s interviewees were keen to spell out their own political values, revealing political and ideological division among the UK-based academics interviewed. This division extended to the extent to which academics felt it important to engage directly with policy processes, or to work at arm’s length from policy, preserving scientific ‘objectivity’. In addition to political ideology, credibility was connected to some methodological approaches, and not others. No interviewee in Smith’s study subscribed to a ‘hierarchy of evidence’ (Petticrew & Roberts, 2003) but most interviewees displayed a clear preference for quantitative methods, and some were dismissive of qualitative methods. As in Bartley’s study, a split was apparent between medical and non-medical academics, and physicians were described as having ‘higher status’ (Smith, 2008: p.193). The relationship between disciplinary and political identity was complex in this study, but at least some of the scientific boundary work seemed to relate to political-ideological divisions (i.e. researchers who found other research politically or ideologically problematic tended to work to undermine the scientific and methodological credibility of that work).

In an expansion of her 2008 study, Smith (2013) compared health inequalities research to tobacco control research, to tease out the complexity of the research-policy interface in population health and to highlight distinction and division among academics regarding orientation toward policy makers, and the role of government. Relevant to this thesis is the prominence of medicalised framings of population health in UK policy settings, which Smith illustrates as powerfully shaping the kind of evidence favoured in policy contexts. Medical framings of population health were apparent institutionally in two ways. First, in the structure and division of policy responsibility (health inequalities policy being an issue for resolution via health policy, not housing, education, or budgetary policy). Second, the medical framing was embedded within the efforts of disease-specific third-sector organisations lobbying for focus on particular diseases (and the absence of any organised lobbying effort for the social determinants of health). Understanding the extent to which similar division is apparent within *academic* HIDR is one aim of this thesis.

Smith’s (2013) data also pointed to tension between researchers with and without medical qualifications, and further division was evident between researchers strongly preferring quantitative (preferably experimental) research and others who desired more interdisciplinary collaboration, including qualitative methods.

However, where Smith’s studies (2008, 2013) highlighted the importance of researchers’ political and ideological views, Bartley (1992), despite setting out initially to investigate researchers’ political stance, concluded that disciplinary training was the more important factor in establishing and sustaining division among researchers on the question of unemployment and health. Whilst my primary aim is not to understand researcher's political views, this thesis provides an opportunity to investigate the balance and possible interplay between disciplinary training and researchers' political stance.

Crucially, all of the studies discussed in this section were restricted to the UK context, and HIDR is much larger than UK-based research activity (Cash-Gibson et al., 2018). This thesis presents an opportunity to investigate the extent to which these divisions, consistent in the UK across multiple decades, are apparent in other contexts.

Garthwaite and colleagues (2016) reinforced the conclusions of these earlier studies with the results of fourteen focus groups involving participants engaged in HIDR, policy, practice or advocacy. Results revealed "obvious methodological tension within the multidisciplinary field of health inequalities" (p.466). Like Smith's (2013) interviewees, some participants criticised the dominance of randomised controlled trials and classical epidemiological methods, arguing important questions cannot be addressed within that framework. Interestingly, a perception that economists were much better at engaging in 'entrepreneurial activities' (as identified by Bartley 20 years prior), emerged in focus groups, with one participant noting:

> "We're not hugely public and most of us don't write commentaries for newspapers or letters to the papers, or do those sorts of media things *that economists do all the time*" (Garthwaite et al., p.471, emphasis added)

The authors identified three 'ideal types' of health inequalities researcher, with distinct epistemological foundations:

> *Policy-focused positivists* expressed commitment to the pursuit of quantitative, experimental research to identify and evaluate effective interventions. This type of researcher prioritised scientific independence and methodological rigour over opportunity to engage with policymakers.
>
> *Empathetic Ethnographers* felt that HIDR needed to pivot in the direction of better understanding the lived experience of health

> inequalities, studying what inequalities mean in peoples' social worlds, and publishing the thoughts of those experiencing disadvantage.
>
> *Critical Materialists* expressed a desire that HIDR move away from attempts to elucidate the precise pathways by which the SDoH contribute to poor health, and instead focus on documenting the extent of inequalities, identifying actors influencing important decisions and describing important power relations.

The Policy-focused Positivists seem reminiscent of the medical statisticians described by Bartley, who deeply valued scientific rigour and felt that maintaining scientific standards was more important than active political engagement. The epistemological commitments of this group appear to generally correspond with epidemiology, however this cannot be stated for certain since the focus of this study was methodological (rather than disciplinary). The Empathetic Ethnographers expressed concerns similar to some participants interviewed by Smith (2013), and also to Syme (1996, 2007) and Popay (1998) when they expressed a desire for 'better' interdisciplinary work including qualitative methods. The commitments of this group seem to align with a sociological epistemology. It is unclear where the final type of researcher, the *Critical Materialist* should fit. The political focus of these researchers suggests political scientists (and members of related disciplines) are also important contributors within HIDR.

Within Garthwaite's study (and others discussed in this section) 'disciplinary training' is a resource in explanation, invoked to make sense of attitudes expressed by researchers, not the subject of specific study. This thesis aims to move beyond appeals to disciplinary training as an explanatory factor that contributes to understanding divisions in researchers' preferences. Instead, in this project, disciplinary training is the central factor of interest, itself considered worthy of dedicated analysis and understanding.

1.6 The Economic Approach

Bartley's (1992) conclusions point to economics as being an influential discipline, and economists as being somehow different to other academics involved in HIDR. However, direct, critical engagement with economic theory is limited within the HIDR literature, and beyond one discussant labelling media engagement as a 'thing economists do', was not reported as a major theme in Garthwaite's (2016) focus groups. How does the economic approach to health inequalities differ from the sociological or epidemiological approach, and what is the consequence of such difference?

A debate at the turn of the new millennium provided a glimpse into how an economic approach to HIDR might differ from sociological or epidemiological perspectives. The groundwork for this controversy was laid in the 1980's with the proposal (Illsley & Le Grand, 1987) that econometric methods (useful for the study of income distributions), could be extended to studying distributions of health. Le Grand (1987) suggested that the traditional approach of analysing health differences between social groups was vulnerable to "the vagaries of classification schemes" (p.183) and that a complementary approach could be borrowed from econometrics, focusing on the distribution of life expectancy among individuals, rather than on differences in average life expectancy between socially-defined groups. Le Grand clarified that this alternate method does not answer the same kind of question as traditional approaches, but nevertheless went on to articulate the somewhat explosive claim that health inequality in Britain was "low" in 1982, and had decreased over time; a direct conflict with the government-commissioned Black Report's (1980) claim that inequality was significant, and had increased. Interestingly, this tension did not precipitate debate about the definition of health inequalities across disciplinary lines. The majority of the citations of this work prior to 1997 were attracted as part of a related but different debate about whether inequality in income directly causes inequality in health, a debate which continues today and will feature in this thesis.

The distinct character of econometric approaches resurfaced in 2000, when a section of the WHO led by economist Emauella Gakidou proposed that "the quantity of interest for studying health inequality is the distribution of health expectancy across individuals in the population" (Gakidou, Murray &

Frenk, 2000:p.42). Gakidou and colleagues argued for a conceptual and operational approach which focused on individual difference, as was suggested by Illsley and Le Grand. The "World Health Report" (WHO, 2000) examined the distribution of health via measurement of what it termed "pure" health inequalities: differences in health outcomes between all possible pairs of ungrouped individuals. This stimulated a spirited exchange between researchers within the WHO and a group of HIDR academics, revealing epistemological commitments on both sides.

Paula Braveman, Nancy Krieger and John Lynch (2000) took issue with the description of inequalities between individuals as 'pure', and described the individual measure as "a technical solution that discards key questions" (*ibid* p.78). Braveman and colleagues expressed concern about the ethical perspective underpinning the individual measure, and found the underlying question ('what factors differ between individuals with better and worse health?') restrictive, and inadequate for setting public health priorities.

The architects of the new measure responded by characterising it as a "better" dependent variable for the investigation of health inequality, and asserting that it facilitates a "more rigorous" analysis of the social determinants, although no explicit justification was provided for these claims (Murray, Gakidou & Frenk, 2000). The defence of the new method, far more technical in tone than the impassioned critique of Braveman *et al* to which it responded, seemed to be underpinned by an underlying belief that understanding health inequalities between individuals is a more valid scientific endeavour than identifying factors which explain social inequalities in health. Murray and colleagues (2000) dismissed Braveman's anxiety that examining the distribution of health among individuals could result in HIDR dropping off the policy agenda, and also rejected Braveman's claim that inequalities between rich and poor will not be fully captured by this new approach. The logic of these claims is clearly not apparent to Braveman, Krieger and Lynch, and the whole exchange seems to suggest two groups of researchers understanding health equity (and, perhaps, "health" and "equity) in fundamentally different ways.

This debate about measurement is a variant of the 'individual vs social causes' debate. The new measure detected differences between pairs of (atomised) individuals, in contrast to traditional approaches which group individuals according to social variables. An individualist ontology is detectable in the writing of Gakidou and colleagues, who question the value of knowledge about socially-defined groups. This particular debate is interesting because it is not an example of the well documented qualitative/quantitative divide within population health (Baum, 1995; Krieger, 2000), but represents diversity of epistemology *among quantitative researchers*, along disciplinary lines. The surrounding discourse is also suggestive. Marmot (2001) characterised the debate as being between economists, to whom this kind of analysis “seems logical”, and “the rest of us” (p.1167) defining health inequalities as occurring between social groups. This othering of economists and economic ideas by one of the most influential HIDR scholars is telling, and combines with the limited engagement with economic theory in published HIDR literature to suggests that economic approaches are viewed as somehow distant or disconnected from other health equity scholarship. This may be partly explained by the belief (documented by Smith, 2015) that particular market and growth-orientated discourses have tremendous power, and are insurmountable in policy settings. Whilst the economic epistemology is not wholly comprised of these discourses, the view that ‘economics’ is an obstacle to policy progress may explain the apparent reluctance to engage with economic theory, in research settings.

1.7 Research Question & Thesis outline

This chapter introduced the topic of the thesis, and discussed diverse approaches to one kind of population health research: health inequalities and disparities research (HIDR). Studies from the UK suggest that disciplinary training potentially helps to explain why certain debates have arisen and persisted within HIDR, rather than resolving. However, epistemological tensions have not been explored in detail, and disciplinary training has not been positioned as a specific focus of study. Existing studies suggest the disciplines of sociology, economics and epidemiology have informed the thinking of authors who have made substantive contributions to the study of health equity. This published material combines with my own experience working in academic departments of economics and epidemiology (and now, more recently, social science) to suggest that these three disciplines conceptualise the determinants of health, and the processes by which these determinants lead to unequal health outcomes in fundamentally different ways, potentially limiting associated dialogue.

Coming into the PhD program I assumed that existing literature would provide me with established reviews of and analytical approaches toward the role of disciplinary training in research, and my task would be the application of existing ideas and methods to HIDR. I quickly discovered that this fundamental work has not been undertaken, suggesting that the role of disciplinary training in the design, execution and interpretation of research is a neglected research topic (explored in the next chapter). In this thesis, I aim to contribute to the development of this area, and also to examine the ways disciplinary training shapes the questions researchers ask and answer within HIDR. Investigation and analysis in this thesis is therefore guided by the following main research question:

What is the substance and consequence of disciplinary difference in research about health equity?

This question is explored via a mixture of quantitative (bibliometric) and qualitative (interview) methods, drawing on data from diverse geographic and institutional settings. The thesis provides rich empirical data exploring the ways researchers with different disciplinary training approach the study of health equity, and population health more broadly. The question is approached analytically via three aims:

> Aim 1: Identify the blend of formal disciplinary training present among health equity researchers, enabling analysis of the distribution of disciplines across the research area
>
> Aim 2: Explore diversity in epistemological approaches to health equity

research, including considering how such diversity corresponds with disciplinary background, manifests in the design and evaluation of research studies, and intersects with other factors.

Aim 3: Assess the impact of disciplinary differences on interdisciplinary and collaborative research efforts within HIDR.

Before moving on to situate the thesis in the broader literature connected to the study of disciplines, this chapter concludes with an outline of the thesis. First, a brief note on terminology: The Anglo-Saxon world distinguishes between science and the humanities, whereas many European countries do not (Weingart, 2012). When referring to 'science' in this thesis, I include all areas of systematic research, as in the German 'Wissenschaft'.

1.7.1 Thesis outline

Chapter 2 steps away from health-related research to situate the thesis within a set of overlapping literatures considering, to varying extents, the role of disciplines within science. Chapter 3 is a brief statement of methodology, providing a rationale for the combination of ideas and concepts in my analysis. Chapters 4 and 5 present the specific research methods I apply, including discussion of challenges and mishaps. The thesis contains 5 empirical chapters, Chapters 6-10. Chapter 6 presents the results of a bibliometric analysis which maps the field of HIDR, and illustrates the diversity of participating disciplines, as well as the distributions of these disciplines in citation-space. In chapters 7 and 8 I draw on qualitative interview data to explore, in detail, the extent to which the disciplinary diversity suggested by the bibliometric analysis corresponds with diverse approaches to research conduct, evaluation and design. Chapter 9 focuses on use of statistical methods, and interpretation of statistical results. Chapter 10 considers the implications of the findings presented in Chapters 6-9 for interdisciplinary research, and details the benefits and challenges of collaborating across disciplinary boundaries. Chapter 11 draws together the findings, to summarise the key epistemological and practical differences between disciplines, and to position these insights with the literature. The thesis concludes with a discussion of this PhD project's contribution to the theoretical and conceptual treatment of disciplines.

Chapter 2: Studying Disciplines, Background & Key Concepts

> [The] modern system of scientific disciplines […] is one of the truly innovative social structures of the modern world.
>
> Stichweh, History of Scientific Disciplines (2001:p.13,729)

2.1 Introduction: Considering disciplines

Anecdotally, a host of ills are attributed to 'disciplinary difference' in research, but the substance of this difference cannot be easily located in the population health literature. A literature search for the co-occurrence of the terms "disciplinary difference" and "health" is overwhelmingly dominated by research on disciplinary variability in teaching practice and student attitudes. In clinical spaces, multidisciplinary teams are an established and well-studied feature of the landscape, however the motives, incentives and challenges for a team delivering care are not necessarily the same as a team conducting research.

In this chapter I introduce and review existing empirical approaches to the study of disciplinary difference, and the established insights upon which this thesis builds. After briefly reviewing the history of disciplined science in Section 2, in Section 3 I discuss four challenges which complicate the conceptualisation and empirical study of disciplines. In Section 4 I introduce four (disciplined) strands of research which have generated the insights, theories and concepts I draw upon; Sociology of Professions, Higher Education research, Sociology of Scientific Knowledge and Science and Technology Studies. In Section 5 I briefly present relevant concepts from the study of interdisciplinary research, and in Section 6 I discuss literature arguing against my claim that disciplines are an important feature of modern science, worthy of special empirical focus.

2.2 The Origins of Disciplined Science

The structures now recognised as scientific disciplines have existed for roughly two centuries. Until the end of the eighteenth century, the *disciplina* performed an archival function, representing:

> "a place where one deposits knowledge after having found it out […] not an active system for the production of knowledge." Stichweh, 2001: p13,728

Sciences, rather than being configured as a set of disciplines independently producing knowledge, were configured as a hierarchical sequence (Stichweh, 2001; Weingart, 2012) which individual scientists sought to scale. This hierarchy was chiefly contained and navigated within national Academies of Science, the dominant scientific institutions of the era. However, as scientific activity became increasingly concerned with the generation, collection and analysis of data (rather than the documentation and interpretation of naturally occurring events) the sheer quantity of information, analysis and commentary would eventually overwhelm these generalist scientific structures.

In the final decades of the 18th century, scientists began to specialise in and organise around particular problems and sets of concepts (De Solla Price, 1963). Such specialisation required a supportive social context, and the educational system provided a setting within which such specialised roles could be institutionalised as occupations (Stichweh, 2001). Therefore, the emergence of disciplines in science was closely coupled to the increasingly structured nature of higher education throughout the 19th century. Institutionally, scientific activity began to shift from societies and academies into universities (Weingart, 2012). One consequence of this shift was the 'closure' of scientific communication: that which was previously presented for the general public was increasingly written for the exclusive consumption of fellow scientists.

Scientific communication also began to take new forms. Where in the early- and mid-18th century a dizzying array of publication formats were evident (Bazerman, 1988), in the 1780s, French, German and (later) English specialised journals emerged (Stichweh, 2001) and the structure of scientific text became more uniform. In this way, disciplinary communities of specialists also became *communities of authors*. By 1850, rather than scaling the hierarchy of faculties, authors were building a new kind of intellectual career, a *disciplinary* career which typically demanded migration between universities for advancement, rather than movement between departments of a single institution.

This increasing specialisation and movement of scientists between institutions occasioned social change. Members no longer (necessarily) knew each other personally, and it became challenging to remain up-to-date with publications in a particular field. The fragmented state of science is the subject of frequent commentary (Porter & Rafols, 2009), however this is not a recent development:

> "A thousand busy ants are producing daily countless details… only concerned to attract attention for a moment and obtain the best price for their goods […] [the] stream of discovery is split into evermore and evermore unimportant trickles"
>
> Emil Du Bois Reymond, 1886:p450

While the 'stream' of discovery continues to split (Leydesdorff et al., 2013), very few empirical studies supply detailed accounts of the growth of a discipline and subsequent specialisation within it (Weingart, 2012). This may be because disciplines are challenging to confront as research objects. In the next section I outline specific challenges which complicate the study of disciplines.

2.3 Five Challenges to a Straightforward Study of Disciplines

The preceding review illustrates the plural functions of academic disciplines. At a basic level, disciplines are unit-divisions of knowledge, but also communication systems, educational systems, professional accreditation systems and systems of socialisation. It is via these diverse activities that disciplines are stabilised within institutions, and within society more broadly

(Stichweh, 2001). However, this plurality poses significant challenge in analysis, and may partly explain why there is no coherent theory of disciplines, no well-defined “Discipline Studies” literature.

The systematic study of scientific activity has many names, ranging from the long-established History of Science and Philosophy of Science, to the more recently established (and biomedically dominated) ‘Meta-Science’. These fields ask questions about science for the purpose of reaching diverse goals (for example, to chronicle, to contrast, to understand or to change scientific behaviour). A recently-established thread of enquiry describes the challenges and professional (dis)incentives for *inter*disciplinary work (discussed in Section 5). However, the conceptualisation of ‘discipline’ in this area is sometimes glossed over en route to definition of inter-, multi-, and trans- disciplinarity:

> The main problem with the notion of ‘interdisciplinarity’ seems to be that many people who use it do not make explicit what exactly they understand under a discipline or when exactly a disciplinary boundary is crossed with what kind of consequence.
> Krishnan, 2009:p6

In sum, even into the 21st century, studying disciplines is “not altogether straightforward” (Becher & Trowler, 2001). Multiple authors refer to “the challenge” or “the problem” of studying disciplines (Hyland 2004, Becher & Trowler, 2001; Krishnan, 2009; Bazerman, 1988), and much of this discussion centres upon the heterogeneous nature of disciplines and the lack of universal criteria for their identification and categorisation. I review this challenge in detail below, as well as four others that I view as complicating the conceptualisation of disciplines in empirical study.

2.3.1 Recognising Disciplines

A diverse array of terms (domain, field, specialisation, specialism, sub-specialism, sub-discipline, segment) signals the challenge of neatly identifying and defining disciplines and their parts. A popular and straightforward approach involves searching for *institutional* markers. Since institutional (i.e departmental) support for disciplines is usually essential

(Weingart, 2012), and the presence of specialised journals a key step in the development of disciplines (Kuhn, 1970), the existence of departments and journals is presented as a method for identifying disciplines (Evans, 1995). Indeed, as Giddens (1976) suggested, social structures exist only through manifestation in practice, and so attention to material manifestations seems sensible. This is the view of disciplines quite recently presented by Higher Education researcher Paul Trowler, whose empirical treatment of disciplines has evolved over 15 years writing on the topic (see Section 2.4).

> Disciplines become apparent in their playing out in the world, in the process of institutionalisation and in the discursive and other practices which give them substance. Trowler (2014:p.1721)

Trowler terms this a *moderate essentialist* view of disciplines, drawing upon Wittgenstein to present local instances of academic disciplines as displaying a strong ‘Family Resemblance’ originating from their ‘common background knowledge about key figures, conflicts and achievements’ (Trowler, 2014:p6) , whilst also allowing for the analytical treatment of disciplines as complex, dynamic and contextually-dependent social phenomena. I adopt this view of disciplines in this thesis. Trowler (2014) also draws attention to the ways in which disciplines as social-structures can wield substantial *generative power,* the power to shape surrounding social structures and social practice. This is significant for research practice, if accurate. However, as I discuss in Sections 2.4 and 2.6, this attribute of disciplines is debated within some literatures.

The view of disciplines as flexible social structures does not necessarily imply that they are fragile. Disciplines (especially the large, old disciplines of interest in this thesis) are remarkably stable, representing social facts (Bazerman, 1988) with which scientists must reckon, and within which scientists are obliged to set their careers, despite the current push for interdisciplinarity (Lyall, 2019).

Some scholars have argued that using institutional markers for the purpose of identifying disciplines is inappropriate, or sub-optimal, because the appropriate focus is the sub-specialty or specialism:

> Specialist areas or segments seem to offer the most appropriate analytic currency […] The heart of the academic enterprise is the specialist field.
>
> Becher and Trowler, 2001: p64

Undoubtedly, specialisms are the social site of knowledge construction. One reason there is so little literature about 'disciplines' may be that scientific controversy tends to be a major focus for scholarship (Rudwick, 1985; Latour & Woolgar, 1986; Bloor, 2011), and controversy typically arises within disciplinary specialties, not within 'disciplines'.

However, specialisms frequently lack the concrete institutional markers of established disciplines (e.g., medicine and economics), and these sub-disciplinary units are, upon examination, *themselves* fragmented into what has been termed 'segments' (Bucher & Strauss, 1961) further complicating analysis. Much conjecture regarding the analytical primacy of disciplines, sub-disciplines, specialisms and segments has proceeded under the assumption that big, old, concrete disciplines are relatively unimportant, and exercise limited influence. For example, Becher & Trowler (2001) agree with Chubin (1976) that the 'Specialty' is a useful concept

> "whose various representations capture better than conventional units of analysis, especially "disciplines", the process and structure of research"
>
> (Chubin, 1976: p.73)

Whilst understanding specialisms is important, and limiting the scope of enquiry to 'disciplines' introduces limitations for exploring the reality of science-in-action, it is surely unlikely that disciplines contribute *nothing* to the process and structure of research. However, the strong focus on specialisms in the empirical literature might suggest that disciplinary structures provide only mute institutional background to activities shaped wholly by sub-disciplinary, specialist concerns.

In this thesis I take the existence of "disciplines" as given, visibilised via institutional markers. This is sufficient for my purpose because the disciplines under study here (such as epidemiology, sociology and economics) are well-established and do exhibit institutional, concrete markers such as departments, professorial chairs and journals. Beyond this, I take an inductive approach to the detection of sub-disciplinary units, described in Chapter 4. I do not assume whether, how or to what extent ‘parent’ disciplines exert influence over the content, process and structure of specialised research activity, as understanding this is one aim of the thesis.

2.3.2 A fragmented, disciplined literature

A further challenge to the approach and definition of disciplines stems from the disciplined, conceptually fragmented nature of the literature regarding disciplines, produced from what Krishnan (2009) refers to as diverse ‘paradigmatic angles’. The conceptualisation of disciplines depends on the discipline of the thinker, and on the purpose for which they are reaching for disciplinary behaviour or identity as a topic or analytical resource (Biagioli, 2009). The various directions from which disciplines have been approached include the Sociology of Scientific Knowledge, Science and Technology Studies, Philosophy of Science , History of Science , English for Academic Purposes, Higher Education Studies, and the Sociology of Professions. Each disciplinary group has brought its own concepts, theories and aims to the study of disciplines, and for this reason there has been a significant amount of overlap and ‘re-inventing the wheel’ (Shapin, 1995). This is not necessarily a problem, as the plural social function of disciplines described in Section 2 suggests the study of disciplines may benefit from (or even require) a combination of analytical lenses. This point is made by Shiela Jasanoff, referring to the study of multi-disciplinary domains:

> “The traditional disciplines encounter frictions in their efforts to focus on phenomena… that seem to demand investigation from multiple perspectives.” (Jasanoff, 2012: p204)

Therefore, in my approach to disciplines I draw on multiple bodies of scholarship. As my aim is empirical, to encounter disciplines as they manifest within health equity scholarship, I draw primarily upon strands which contain examples of empirical study of research contexts; chiefly, the Sociology of Scientific Knowledge, Science and Technology Studies and

Higher Education Research. Among these, the thesis is grounded intellectually within the Sociology of Scientific Knowledge, as practiced within what is referred to as the Strong Programme ('Strong Programme SSK', discussed in Section 4).

Contributions from other disciplines (Philosophy of science, History of Science, English for Academic Purposes) enter the thesis via their overlap with these three fields. For example, English For Academic purposes, a sub-specialisation of linguistics, contributes to my conceptualisation of disciplines via examples which overlap with the Sociology of Scientific Knowledge and Science and Technology Studies. I discuss the theoretical basis upon which such diverse traditions can be coherently combined in the next chapter.

2.3.3 The Cognitive and The Social

Once identified, there is the question of where a discipline begins and ends. While the knowledge produced by a discipline (frequently termed 'the cognitive') is closely coupled to disciplinary community and culture ('the social'), these are not one and the same (Kuhn, 1970). Said another way, the "tribe" is not the "territory" (Becher & Trowler, 2001). Disentangling analysis of disciplinary knowledge from the community generating and evaluating that knowledge is a major challenge, and also the site of disagreement between fields (Shapin, 1995). Specifically, diversity is apparent concerning the extent to which the separation of cognitive from social is desirable, and/or achievable. This diversity has clear disciplinary underpinnings, which I unpack below.

Within the Philosophy of Science it has been uncontroversial to separate scientific reasoning ('the cognitive') from the social influence upon science ('the social') and viewed as desirable to draw a bright line between what is 'external' to science and what is 'internal'. This distinction was especially bright within the *logical positivist* tradition, wherein science is viewed as being guided by rationalism or logic (and not by social or cultural influences). A key development in the 20th century was the emergence of a descriptive history of science as counterpoint to this normative philosophy of science (discussed in Section 4), which pushed against idealised models of how science *should* be practiced in favour of accounts describing how science is practiced. The normative slant endures within the Philosophy of Science (Kaiser, 2019), although more recently, the field has undergone what has been referred to as a "practice turn" (Soler et al., 2014), with increasing consensus that "philosophical theories about science must account for how science actually is done" (Kaiser, 2019:p.36). Thus, 'Philosophy of Science in Practice' has emerged as a sub-specialty within the Philosophy of Science (Ankeny et al., 2011). Despite this development and increasing attention to social norms in science, a normative undercurrent remains detectable. Kaiser (2019) notes that much of this work proceeds on the basis of normative assumptions, including assumptions about what constitute "good" examples of scientific practice. Similarly, descriptive, empirical studies are often rounded out with (normative) advice about how science should be conducted (Woodward, 2005; Craver, 2007), suggesting that the cognitive and the social continue to stand as separate, separable phenomena within the Philosophy of Science, and that 'the social' remains positioned as an unfortunate contaminant of 'the cognitive'.

In Higher Education studies, the cognitive and the social tend to be conceptualised as separate, however the utility of this separation in analysis is actively questioned. Becher and Trowler (2001:p.30) discuss 'inter-relationships between cognitive and social aspects of the academic enterprise' while noting that it is extremely challenging to analyse these separately, or to determine a single direction of influence. Becher (1987) concluded 15 years earlier that "it is not productive to try and separate them" in analysis. Nevertheless, 'they' (plural) are understood to *exist* as theoretically distinguishable objects.

In contrast, some scholars within the Sociology of Scientific Knowledge and History of Science take a stronger position, challenging the conceptualisation of science as having discrete cognitive and social elements:

> "The problem of cognitive order *is* the problem of social order […] These are not two things […] they are one thing described from different points of view" (Bloor, 2011: p5)

Here, disciplinary knowledge is itself viewed as a product and manifestation of socially-situated norms and forces.

These conceptual differences reflect the differing fundamental commitments of the disciplines participating in research about disciplines, and further amplify the disciplined character of the literature surrounding disciplines, minimising the extent to which authors in one disciplinary strand can apply concepts or draw upon insights from the other strands.

2.3.4 The Dominance of Physics & Natural Sciences

Not all disciplines have enjoyed the same degree of empirical enquiry (Becher, 1987; Swales, 2001). Physics, while itself containing a diverse set of sub-specialties, has received an outsized focus compared to other disciplines (Bazerman, 1988; Trowler, 2014). This may be because of its status as the "queen" of sciences through the mid-to-late 20th century (Hacking, 2002). Many empirical studies focus exclusively on physics (Galison, 1997; De Lozano & Cardenas, 2002; Bloor, 2011; Shapin & Schaffer, 1985) or include physics among a small number of other disciplines (Ruscio, 1987; Becher, 1987; Knorr-Cetina, 1999). Disciplines other than physics are typically natural or biomedical sciences (e.g. Knorr-Cetina, 1999; Latour & Woolgar, 1986; Gilbert et al., 1984; Rudwick, 1985) leaving the social sciences and applied fields quite thinly represented. Whether and to what extent concepts and conclusions from case studies of physics (and natural sciences) can be unproblematically extended to the social sciences, or to applied disciplines like Epidemiology, is therefore unclear.

2.3.5 Disciplinary Contexts: Research, Teaching and Practice

Although the teaching and research contexts are connected (Barnes, 1982), and much research takes place in teaching settings, "disciplines as articulated in a research context are different than when articulated in learning and teaching contexts" (Trowler, 2014: p1724, and also see Maton & Muller, 2007; Ashwin, 2009). Therefore, conclusions drawn about disciplines from the study of teaching may not necessarily transfer to research in a straightforward way, and it is important that studies of disciplinary difference are clear about the context(s) to which they relate.

Philosophy of Science, History of Science and Sociology of Scientific Knowledge have generally tended to focus on research contexts. Within Higher Education research, the empirical literature concerned with disciplines tends to focus on teaching (Neumann & Becher, 2002). However, studies of *inter*disciplinarity within Higher Education Research (discussed in Section 2.5) illustrate why teaching and research may be appropriately handled as separate academic domains. While interdisciplinary research is occurring (somewhat) and is generally recognised as valuable, interdisciplinary teaching remains a "nut" universities are trying to "crack" (Lyall, 2019). The incentives and reward structures driving interdisciplinary research do not appear to be driving interdisciplinary teaching, underscoring the separation of these activities within the academy, and perhaps pointing to diversity in the generative power of disciplines across contexts, even within a single institution.

The same emphasis on educational contexts is present within the sub-specialty of Linguistics known as English for Academic Purposes (EAP). In the edited volume "Academic Discourse Across Disciplines" (2004) Hyland makes a strong case for the role of disciplinary influence on the deployment of language in science, and the ways in which "rhetorical practices are inextricably related to the purposes of the disciplines" (Hyland, 2004: Ch1, p.36). However, in the same volume, Shaw (Ch3, p.103) notes that "much of the existing [EAP] literature is based on educational genres" such as Masters dissertations and PhD theses, leaving the analysis of research texts somewhat neglected.

A third context within which disciplinary difference is relevant is that of "practice". This last dimension seems particularly confused, as within the

Sociology of Knowledge, Science and Technology Studies and Philosophy of Science, “scientific practice” is typically employed as a synonym for research activity (E.g., Kuhn, 1962; Barnes, 1982; Knorr Cetina & Reichmann, 2015; Rudwick, 1985). This includes the diverse set of ‘practices’ which make up research, including ‘social practice’ (Bloor, 2011), ‘knowledge practice’ and ’expert practice’ (Knorr-Cetina, 1999), ‘craft’ and ‘localised practices’ (Latour & Woolgar, 1986). However, “practice” may also refer specifically to *applied research,* set up in opposition to theoretical or pure research (Bloor, 2011:p.136 ; Becher & Trowler, 2001: Ch2). A further meaning concerns disciplines linked with *vocational* practice, such as Medicine, Teaching and Law (Campbell & Wiles, 1976; Becher & Trowler, 2001:Ch3). Finally, within non-medical fields, studies of non-research practice might be identified via the term ‘professional practice’, however, in research about health, ‘practice’ refers typically to *clinical* practice and not to research, either pure or applied.

Of course, there is no unified academic or scientific “practice”. However, such diverse meanings obstruct the straightforward mapping of concepts from one (disciplined) strand of research about disciplines onto another.

This thesis is concerned exclusively with the research context, which may strike the reader as a narrow framing. I focus on research for two reasons; First, because the underlying cause of disciplinary difference is assumed to be located in the content and form of disciplinary knowledge, which, while perhaps evident in discussions of teaching and policy, originates from and is rooted most deeply within the research context.

Second, for the disciplines involved with the study of population health, disciplinary knowledge (as constructed within the research domain) is the well from which a discipline’s generative power is drawn. It is no accident that disciplines are generally referred to as “knowledge communities” rather than “teaching communities” or “writing communities”. The heroic myths (Taylor, 1976) around which disciplines grow are rarely tales of excellence in teaching (rarer still are the legends of efficient organisational management); the ‘giants’ upon whose shoulders disciplines sit are almost invariably giants of research (including originators of new theoretical and philosophical approaches). For this reason, disciplinary differences in the interpersonal dynamics of teaching, language, institutional culture and management seem very likely to reflect the way disciplinary difference manifests in research settings, and a detailed understanding of the latter may help to explain the former. However, the recent study of disciplines has generally turned away from research settings and knowledge construction, a turn discussed in detail in the next section.

2.4. Research about Disciplines: Major Strands & Contributions

In this section I present three strands of research that have established key insights regarding the general ways disciplines tend to vary. A recurring theme in this review is a dearth of enquiry regarding research activity and knowledge construction in contemporary research. While I do not present a systematic appraisal of these three fields, I present peer-reviewed accounts generated within each field suggesting that analysis of research activity and knowledge construction is minimal, and/or is declining. In addition, I am not aware of any research specifically focused on the function of disciplines within population health research. Collectively, these trends across multiple research domains point toward the gap filled by this thesis.

2.4.1 The Sociology of Professions

Since disciplines are communities of professionals, sociological insight into the study of professions and professional groups is potentially relevant. Sociological concern with medical professions (and professionalism more broadly) attracted the focus of prominent figures within sociology in the early and mid-20th century (Merton, 1958; Parsons, 1951). These approaches to professions tended to focus on the traditional professions of medicine, law, accounting, architecture, the clergy, science, and engineering (Gorman & Sandefur, 2011). Analysis has two major thematic foci (Adams, 2015), i) professions as occupations, professional work and labour markets; and ii) professions as a component of social order and social regulation (Sciulli, 2009).

Gorman and Sandefur (2011) identify four key attributes of professionalism to emerge from the 'Golden Age' of enquiry into professions dating from 1960-1990. First, professionals possess and employ *expert knowledge,* composed of formal abstract principles (Abbott, 1988). In controlling that body of knowledge, professionals exercise *technical autonomy*, as "no one outside the profession can legitimately dictate what those professionals do or how they do it" (Gorman & Sandefur, 2011: p.180). Professionals also share a normative *orientation toward service*, and receive high *status, income and other rewards*.

While these four themes remain visible in contemporary study, 21st century approaches are less concerned with defining or ring-fencing "the professions" from non-professions. More important today is the demarcation of jobs and occupations requiring expert knowledge ('knowledge workers', Adams 2015) from those which do not, and investigation has shifted toward themes of inequality, organisational structure, insecurity in employment, power and control of employees, ethics, and identity (Gorman & Sandefur, 2011). Researchers from diverse sociological traditions consider the role of professions, including the sociology of work, inequality, medicine, law, and organisations. In addition, there is a clear difference in research focus between the USA and Europe, especially in the area of regulation, frequently a focus of British and European work, but rarely examined in the USA (Adams, 2015).

While this thesis is not grounded intellectually within the sociology of professions, my analysis proceeds with an awareness that most academics are employed by large, complex organisations, whose structures and policies influence research activity. Departmental structure and strategic aims impact the kind of research academics can do (Spurling, 2012). In addition, funders and other third-parties such as governmental agencies "make their presence felt" (Gorman & Sandefur, 2011) in ways which drive or dampen research on particular topics, or via particular methods.

The extent to which 21st century professionals possess technical autonomy is a relevant point of contestation. On one hand, the existence of the autonomous professional is questioned, viewed as a relic of the past undermined by social change (Dixon-Woods et al., 2011; Leicht & Fennell, 2001), but, in some fields (notably medicine) the discretion of professionals appears to have been maintained in some contexts (Evetts, 2002). This question feeds into a strand of research regarding employers' efforts to control workers and the extent to which workers accept or resist that control. In medicine, doctors have been shown to employ creative strategies to "do what they want" (Hoff & McCaffrey, 1996) and this may perhaps extend to academic and research settings.

Professionals (including scientists) should not be treated analytically as homogenous groupings, and contemporary studies attribute variation within groups of workers to factors such as social background, diverse and stratified training, organisational cultures, and the emergence and proliferation of new work statuses (Granfield, 2007; Schleef, 2005). This thesis therefore proceeds with awareness that scientific groups are likely to exhibit this same diversity, in addition to diversity attributable to disciplinary background.

Health care professions (especially nursing and medicine) seem the most intensively studied occupations (Adams, 2015). While the sociology of medicine is of relevance to population health research, a sociological account of medical practice is not an account of research practice. In the Sociology of Professions generally, specific interest in expert knowledge is declining; Adams (2015) summarised 500 published articles containing the phrases 'sociology of professions' or 'professional employment' and grouped these into 10 thematic areas. Of the ten themes, the "Knowledge and expertise" theme was the most thinly represented, at just 6% of publications, and this theme ranked last in all geographical regions except Europe, where it ranked second-last in terms of output.

This suggests that sociological concern with workers and occupations is not currently focused on the development and application of knowledge. This is further reflected in the exclusion of the research domain from discussions of employees who solve ‘concrete problems’:

> Most knowledge workers—**other than those engaged solely in research and teaching**—make use of expert knowledge to solve concrete problems.
>
> Gorman & Sandefur, 2011: p281-282 (emphasis added).

Two well-known books cut against this trend, taking scientists and their expertise as a special focus. This section concludes with a summary of relevant material from Michele Lamont’s *How Professors Think* (2009) and Andrew Abbott’s *Chaos of Disciplines* (2001).

How Professors Think

Michèle Lamont observed grant peer-review panels to investigate evaluative culture(s) within academia and the processes via which academic standards of excellence are agreed upon in six disciplines; Philosophy, History, Anthropology, English Literature, Political Science, and Economics. In addition to observing deliberations, Lamont interviewed panel members, inviting frank appraisals of scientists’ own (and others’) fields to gain insight into the deeper commitments of these disciplines.

Drawing upon Whitley and Bourdieu, a recurring theme was the way in which disciplines

> “shine under different lights, are good at different things, and are best located on different matrixes of evaluation, precisely because their objects and concerns differ so dramatically.” Lamont (2009) p.9

Knowledge seems to have a different character in these six fields, and these diverse knowledges in turn demand diverse analytical approaches, and methods. Lamont concludes that epistemology and notions of quality are not parallel or separate features of disciplines, but that they are linked, and diversity in academic criteria for excellence is a consequence of epistemological diversity.

Lamont identifies four “epistemological styles” (p57-58) employed when evaluating grants; which she labels comprehensive, constructivist, positivist, and utilitarian. These styles are summarised in Table 1, below. The comprehensive style values the study of wholes, attention to detail, and contextual specificity. The constructivist style emphasises proposals which witness the lived experience of various groups, and values reflexivity. The positivist style favours generalizability, and hypothesis testing. The utilitarian style resembles the positivist style, but with clear preference for the production of instrumental knowledge. Lamont's aim was to investigate the diverse evaluative cultures within six humanities and social sciences, and for this reason Lamont did not connect these epistemological styles to the content of the disciplines concerned, or speculate why this diversity arises, or persists. To my knowledge Lamont's epistemological styles have not been specifically investigated or searched for in the context of health-related research, although these do map neatly onto the 'types' of health inequalities researcher described by Garthwaite and colleagues (2016, see Section 1.5).

Epistemological Style	**Favoured Attributes in Evaluation**
Comprehensive	Strong rational and theoretically informed agenda Values verstehen (study of whole phenomena, not decomposition into parts), attention to detail, complexity and contextual specificity.
Constructivist	Emphasizes proposals that "give voice" to various groups, values reflexivity
Positivist	Favours formal models, generalizability, testing or disproving theory, and hypothesis testing.
Utilitarian	Resembles the positivist style (above), but with clear preference for instrumental knowledge and attention to "real world" problems.

Table 2, Lamont's Epistemological Styles (Simplified from Lamont, 2009: p.176)

Another important difference between disciplines emerging in Lamont's study was related to their varied capacity for consensus regarding definitions of quality. Historians and economists were distinctive for their ability to agree, rapidly, on what kind of work was "excellent". As economics is a discipline featured in this thesis, Lamont's explanation for this (which agrees with my own experience as an undergraduate student) is of relevance:

> Economists' cohesion is grounded in a cognitive unification that was largely achieved by the 1960s, as mathematical economics triumphed over other approaches (institutionalist, Marxian, and anti-mathematical institutionalist, for instance). This ascendance of mathematical economics has translated into a homogenization of the core courses in every major institution […] Perhaps owing to their discipline's epistemological cohesiveness, economists seem much less concerned with (or even aware of) the constructed nature of excellence.
>
> Lamont, 2009:pp.100-101

English Literature and Anthropology stood in contrast to the cohesive disciplines of History and Economics. From the outside, these fields were perceived as existing in constant turmoil, seeming to perpetually debate the nature and content of the field.

This apparent difference in disciplinary cohesion is a recurring feature within the literature about disciplines. Two other key differences identified in Lamont's study were the role of subjectivity in the pursuit of knowledge, and disciplinary preference for pure versus applied insight.

In analysing the process via which multi-disciplinary panels arrive at consensus, Lamont identified a necessary precedent to consensus which she terms *cognitive contextualization*: the use of "criteria of evaluation most appropriate to the field or discipline of the proposal under review" (p.106). Panel members reached consensus by applying different definitions of quality to different proposals, and this required them to mute their own "disciplinary prejudice" (p.135).

This flexibility was achievable only within settings which engendered trust and respect between participants, actively managed by a skilled panel chair. Cognitive contextualisation may be more difficult in research settings, and harder still when decisions relate to scholars' own projects, not proposals from individuals whom they do not know, and will never meet. In particular, population health researchers are likely to be competing for the same pools of funds, which may introduce a competitive dynamic not observed in Lamont's study.

Chaos of Disciplines

Andrew Abbott (2001) uses the discipline of sociology to frame a general argument about the self-similar nature of cultural distinctions, and to advance a general account of how knowledge changes in the social sciences. As sociology is a discipline which features in this thesis, Abbott's insights into the nature of knowledge in sociology are relevant.

Abbott notes that, unlike some other fields with 'strong boundaries', sociology has no intellectual basis for excluding strands of enquiry which claim to merit sociological attention, and that this produces extreme diversity, 'not one sociology but many' (Abbott, 2001:p4). Such diversity is brought about via distinction (e.g., "Culture" vs "Social Structure") repeated at multiple levels. 'Fractal distinction' refers to the way in which, for example, the culture arm of the 'Culture vs Social Structure' distinction in sociology itself contains a Culture/Social Structure split, visualised in Figure 2. Abbott argues that Self-Similarity, 'the idea that a subset of a larger unit can contain scaled-down versions of structures and processes in that larger unit' (p.3), is an important general feature of social structure, and provides examples of theoretical and methodological distinctions cascading as in Figure 2 from history, sociology, and literature.

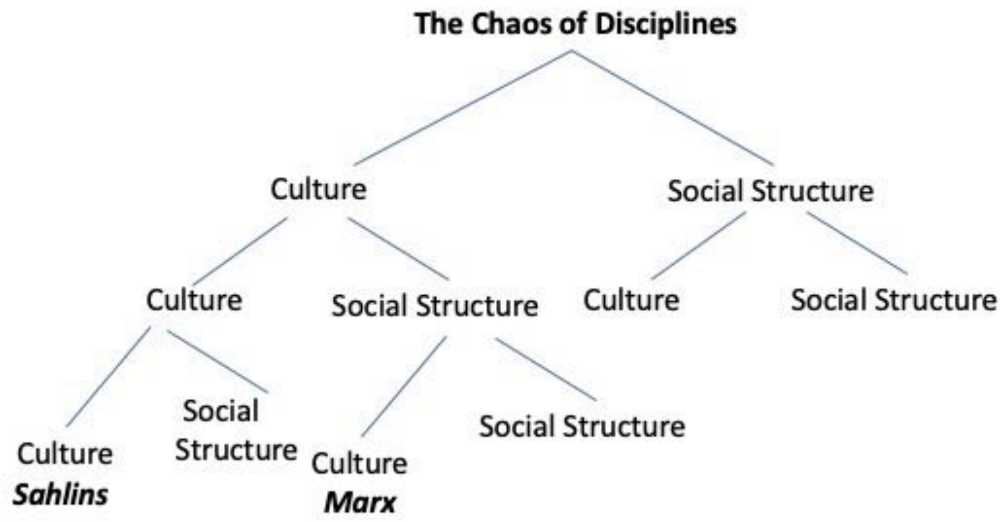

Figure 2 - Sociology is characterised by a set of repeating, self-similar distinctions. (Figure 1.3 from *Chaos of Disciplines*)

Abbott presents this intellectual-crawl-via-repeated-distinction as giving rise to a cultural tendency for ignorance regarding the distant limbs of a discipline's fractal tree, and to an associated tendency for scientists to "know one's close relatives well and one's distant relatives not at all" (p.20).

Abbot's central conclusion is that the social sciences are destined to cycle around a repeating pattern of concerns and principles, rather than to "progress" in a linear manner. Indeed, in Sociology, ninety-one articles or books have "brought something or other 'back in'" since 1964 (p.16). The tendency for certain disciplines to return to fundamental questions has been noted by other authors (most specifically Becher, 1987), and Abbott's conclusion that the content of the social sciences is cyclical is consistent with Lamont's study, wherein one interviewee noted that they are wrestling

> with very traditional problems that [have] defined the subject for, you know, thousands of years. It's not that entirely new problems come up that haven't been studied or investigated before. (Lamont, 2009:p.69)

In this thesis, disciplines which repeatedly circle back to foundational concerns are referred to as disciplines possessing *integrative codes*, after Bernstein (1971, discussed in Section 3.4.2).

Abbott's attention to the function of self-similar, cascading distinctions in science is a unique contribution, with applicability and consequence for the disciplines making up population health. For example, there is a distinction within epidemiology between clinical epidemiology (concerned with biology, disease process, and the efficacy of medical treatments) and population epidemiology (concerned with environmental exposures, non-modifiable risk, and the impact of health policy or population-level interventions). However, according to Abbott's thesis, each of these sub-fields should be expected to contain its own clinical/population split. Status as "clinical" is likely to be *relative*, determined by the degree of clinical-ness usual in a specific setting. In another example, while researchers might distinguish themselves as "qualitative" or "quantitative", I should expect to find quantitative scholars who lean qualitative, and vice versa. Such distinctions can also unfold simultaneously across disciplines. Abbott provides the example of "Historical Sociology" and "Social-Science History", independent products of the History/Sociology distinction playing out in different disciplines. Examples of this phenomenon within population health might be "Medical Sociology" and "Social Epidemiology" or "Genetic Epidemiology" and "Population Genetics".

In sum, the sociology of professions provides important background understanding regarding the conditions which define the role of modern academics, and also contributes to my conceptualisation of disciplinary difference in both a specific (e.g. Lamont's epistemological styles) and general (Abbott's fractal distinction) way.

2.4.2 Higher Education Research

Higher Education Research is a heterogeneous field of study concerned with the Higher Education system, the role of that system within society, the theory and practice of higher education, and higher education policy. The field does not have a precise or global definition (Teichler, 2015) and its status as a "discipline" is debated (Tight, 2020).

A recent review identified 86 journals specific to Higher Education research (Tight, 2020). Like the sociology of professions, Higher Education Research tends to have a national or regional focus. Studies with a global focus or involving international comparison are not frequent (Kosmützky & Krücken, 2014) and the size of the field also varies across countries, being very strong in US and China but comparatively less-visible in European countries. The role of the English language within a country also determines the visibility of literature relating to that country (Teichler, 2015).

Thematic Areas

Jung & Horta (2013) present Higher Education as involving two major themes; 1) Teaching and Learning*,* and 2) Education policy and organisation. Other analyses describe the field as having three major themes; 1) Teaching & Learning, 2) Governance & Management, and 3) The Higher Education System and its Social Context (Tight, 2012).

Teichler (1996) described four "Spheres" of Higher Education knowledge; (1) Quantitative-structural aspects, (2) Knowledge and subject-related aspects, (3) Person-related and teaching and learning-related aspects and (4) Aspects of institution, organisation and governance. 'Research' does not appear in any of these two, three or four-themed summaries of the field, suggesting it is not a major focus.

Tight (2012) pursued a summary of Higher Education publications within specialised journals, organised via eight themes: Teaching/Learning, Course Design, Student Experience, Quality, System Policy, Institutional Management, Academic Work, Knowledge and Research. Of 567 publications reviewed, just 2% (n=15) fit within the "Knowledge and Research" theme, reinforcing a sense that the construction of knowledge in research is not a central concern within Higher Education Research. Furthermore, a longitudinal survey of members of the Consortium of Higher Education Researchers undertaken in 1992 and 2012 (Kehm & Teichler, 2013) found that while substantially more researchers expressed an interest in the themes of international mobility, governance, and management in 2012 than in 1992, interest in 'Knowledge and Research' shrank over the two-decade study period.

Becher & Trowler's book "Academic Tribes and Territories: Intellectual inquiry and the culture of disciplines" (1989) and the expanded second edition (2001) cuts against this trend, and material from the second edition relevant to this thesis is summarised below, followed by discussion of the follow-up publication "Tribes and Territories in the 21st century".

Academic Tribes and Territories

Tribes and Territories built upon early work specifically concerned with the academic profession and higher education system (Clark, 1987). Becher and Trowler (2001) drew on data collected in the mid 1980's in the US and UK to describe a turbulent, marketised, heterogeneous Higher Education system, wherein disciplinary knowledge structures condition, even determine, the behaviour and value-systems of academics.

The 'Tribe and Territory' metaphor in the title is employed throughout, referring to intellectual boundaries as demarcating "territorial possessions that can be encroached on, colonised and reallocated" and contrasting disciplines with strong and weak "borders" and senses of "nationhood". The central question is how the nature of knowledge (the territory) is related to the cultures of those who explore it (the tribe). However, the analytical focus is more squarely upon the latter than the former.

In agreement with the findings of How Professors Think (Lamont, 2009), a key variance between disciplines to emerge in Tribes and Territories is the extent to which consensus and agreement regarding quality, questions, and methods is possible. Fields with strong boundaries tended to display predictable cultural characteristics:

> "The more closely defined and better-defended the boundaries are between hard specialisms, and the more tightly knit the groups associated with them, the easier it is to maintain the integrity of received doctrines by the ostracism or expulsion of internal dissidents and the refusal to provide entry permits to outsiders with dubious credentials"
>
> Becher & Trowler, 2001:p.85

Becher and Trowler's findings reinforce the plural character of disciplines, and demonstrate that, within the institutions studied, teaching tends to be undervalued, that ‘hard knowledge domains are regarded more highly than soft ones, and pure than applied’ (p.81). Foreshadowing Abbott’s discussion of self-similar distinctions in academic culture, institutional preference for pure vs applied knowledge was also shown to manifest *within* disciplines: “Mathematical economists were considered the creme de la crème” (p.81). Additionally, bid-and-deliver funding models were presented as having epistemological impact, although the specific consequence of this impact on research projects was not explored in detail.

Academia as a profession emerges as being distinctive for its constant gradings and rankings: of journals, institutions, departments, and students. The ‘pervasive process of evaluating intellectual worth’ (p.82) is likewise deeply ingrained within the culture of most academic institutions.

Becher & Trower’s analysis also suggests that individual eminent scientists matter in academic culture, in *all* disciplines. Self-amplifying processes surround these elite individuals, who are visible and so attract citations, grants and invitations, which further increases their visibility.

These findings may seem, in 2020, to be rather obvious. However, Tribes and Territories represented an early empirical approach to modern academic and disciplinary culture as situated within universities, and the international scale of the study remains unusual within Higher Education research, and the study of disciplines (Tight, 2012). However, while affirming the importance of disciplines in shaping academic culture, Tribes and Territories says little regarding the importance of disciplines in shaping approaches to research, and is not a study of the ways in which disciplinary training shapes the design, conduct and evaluation of research output.

In the 2012 volume edited by Paul Trowler; *Tribes and Territories in the 21st century: Rethinking the Significance of Disciplines,* three essays are included under the “Disciplines and Research” theme which purportedly analyse the enduring importance (or unimportance) of disciplines in research practice (globally, and within law, art and design, and sociology). Despite appearing under the banner of research practice, these contributions are focused on disciplinary differences in the balance between time allocated to research and teaching, debate regarding the purpose of research, diversity in educational ideology, and in departmental culture. In addition, this work is not comparative, preventing the use of one discipline to learn about another, limiting conclusions to the particular disciplines studied.

In the same section, Brew & Manathunga (2012) advance a global critique on the salience of disciplines in 21st century scholarship, and

> "suggest that disciplinary boundaries are no longer of great significance […] other drivers have taken over and interdisciplinary has become the leitmotif of this century. Territorial boundaries have largely gone - insofar as they ever really existed." (p.41)

In this book chapter, the rise of interdisciplinarity is positioned as a "liberating force" supporting academics to escape their disciplinary "cages" which serve to control and regulate them. To support this claim, Brew & Manathunga note that their interviewees tend to adopt a range of disciplinary identities depending on context, suggesting that statements like 'I am a sociologist' do not serve scientists well. This flexibility is interpreted as signalling "a diminution of the significance of professional identity associated with a particular discipline" (p.47), and is linked to what the authors perceive as a "decline in the relevance of distinct disciplinary knowledge in research" (p.42).

Evidence presented for these claims is sourced from collaborative projects which produce "Mode 2 objects" (Mode 2 science is discussed in Section 2.6). Within such projects (e.g., the Human Genome Project) disciplines are "completely ignored" and are "no longer of relevance" (p.46). As the majority of scientists do not work on (or train within) bleeding-edge, trans-disciplinary, multi-year projects, I question whether such projects rightfully serve as meaningful examples for elucidating the role of disciplines, and disciplinary identity in 21st century science.

Brew & Manathunga question the metaphor of disciplines as tribes occupying territories, as an analogy which "traps us in the past", preferring a vision of disciplines as more fluid entities. Brew & Manathunga argue that

21st century knowledge and its complex and multi-layered problems require interdisciplinary approaches, and ways of thinking (and being) which go far beyond what traditional, disciplined science can support.

As I will argue in Section 6, writings which present visions of interdisciplinary science as being more-fit for the complexity of modern life than traditional, disciplined science should not be confused with empirical confirmation that scientists are able, prepared, or prefer to work in an interdisciplinary manner. Analyses which present disciplines as constrictive intellectual “cages” also downplay the advantages of disciplined scholarship, documented within Science & Technology Studies (STS) for over 50 years.

2.4.3 Science & Technology Studies

21st century STS is the fusion of two 20th century streams of scholarship (Jasanoff, 2012):

1. The study of the *nature of science*, scientific practice and technology as social institutions. These institutions possess distinct structures, practices and discourses which change over time and vary across cultural contexts.

2. The study of the *impact and control of science* and (especially) technology, with a particular focus on associated risks to health, safety, democracy, development and the environment.

As the first tradition is itself divided into three strands, a figure is helpful. Jasanoff’s (2012) account of the development of STS is visualised in Figure 3, depicting the ‘impact and control’ strand on the left, and the ‘nature of science’ strand on the right, itself comprised of three research traditions: Strong Programme SSK, ethnographic studies of science, and cultural studies of science.

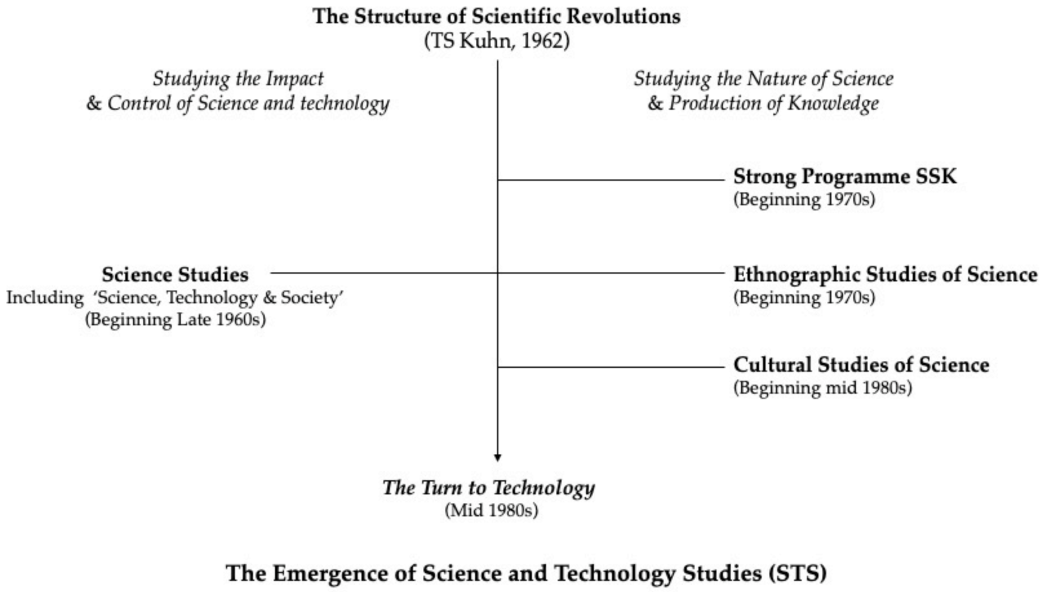

Figure 3, the Emergence of STS, my visual summary of Jasanoff (2012)

Intellectually, this thesis is located on the right hand side of Figure 3.

The Structure of Scientific Revolutions

The emergence of STS is generally understood to have begun with Thomas Kuhn's *Structure of Scientific Revolutions* (Kuhn, 1962 hereafter, *Structure*). *Structure* presented a challenge to the presupposition that science is united and undergirded by a cumulative, unbroken set of facts. Kuhn challenged the 'central metaphor' of positivism (Galison, 1999), that empirical observations stand independent from the theory, instruments, and scholars who generate and interpret them.

In the 1950's and 1960's, historians (including Kuhn) accumulated examples of scientific theory changing in advance of experimental data, and observations and experiments were retro-fitted to agree with emerging theory. These examples suggested that observations did not underpin science like a bedrock, but that observations themselves were theory-laden.

In *Structure,* Kuhn argued this point via multiple historical examples from the natural sciences. In the Kuhnian model, observation does not continue unbroken underneath evolving theory, because new theory occasions the understanding of new observations, but also updated understanding of 'old' observations. This view of theory as shaping both what is observed and how observations are interpreted represents Kuhn's seminal, originating contribution to STS (Hacking, 2012; Barnes, 1982; Galison, 1999).

Therefore, the context and circumstances surrounding theory-change emerged as objects of scholarly interest in the 1960's. In the following decades, society and culture entered the study of science, and the boundary presumed to separate 'science' from 'society' began to perforate under scrutiny. Three streams of 'nature of science' STS (Figure 3, Right-Hand side) emerged during this period, tackling this new problem-set via diverse methods.

Strong Programme SSK

Structure paved the way for the treatment of scientific facts as products of scientists' own beliefs, theories, and contexts. British scholars began to probe the extent to which questions about the nature of science (previously the domain of philosophers) could be re-framed and answered using sociological and historical methods (i.e via empirical investigation). This was the basis for a distinctively British Sociology of Scientific Knowledge (SSK), located primarily in Edinburgh and Bath. This strand of SSK sought to render social what was previously considered epistemic, an aim met with fierce opposition from several philosophers of science, who also criticised Kuhn (see Section 2.4.4.5). In this thesis I draw upon the methodological tenets of Strong Programme SSK (outlined in the next chapter) to set up my approach to disciplines.

Ethnographic Studies of Science

Around the same time, detailed ethnographic studies of laboratory scientists began to emerge in print. An early, influential example is Bruno Latour and Steven Woolgar's 'Laboratory Life: The Social Construction of Scientific Facts' (Latour & Woolgar, 1986). Latour was embedded within a molecular biology laboratory for two years, and Latour and Woolgar used the resulting ethnographic data to study the motivations and 'currencies' appearing to drive scientific activity, the mechanisms by which scientific claims are transformed into scientific facts, and the central role of technology. Specific concepts from Laboratory Life are employed in this thesis, specifically in Chapters 9 and 10.

With colleague Michael Callon, Latour continued to critique the distinction between the human and non-human (or, social and material) elements of science. This work is the basis for Actor Network Theory, and foregrounded technology as being worthy of careful and dedicated study, a focus which continues in STS today Jasanoff (2012) as well as within geography (Truffer, 2008) and sociology (Gunderson, 2016).

Science and Technology as Cultural Formations

The third strand of ‘nature of science’ STS emerged in the 1980’s, and positioned science and technology as cultural formations. Drawing on the humanities, anthropology (e.g. Haraway, 1989), feminism (e.g. Fox Keller, 1986), and theorists of language and power, this strand was distinctive for its focus on the meanings attached to science, and to scientific products. An influential output of this strand is Karin Knorr-Cetina’s (1999) *Epistemic Cultures*. Knorr-Cetina conducted simultaneous ethnographies in two settings, High Energy Particle Physics and Molecular Biology, revealing diversity in the underlying cultural ’machineries of knowing’. Specific concepts and theory developed in *Epistemic Cultures* are applied in this thesis, in Chapters 7 and 8.

Science Studies

The second broad tradition within STS (Left hand side of Figure 3) is concerned not with the nature of science or construction of knowledge, but with its interface and impact upon society, politics and culture. Following the conclusion of the Cold War, students worldwide began to advocate for scholarship of scientists’ complicity in war and the impact of technology on society. Several centres of ‘Science, Technology and Society’ were established in the late 1960’s, motivated by concerns that science and technology were developing in ways contrary to the public interest (Jasanoff, 2012). Study of science and technology policy also gained momentum during this period. This PhD project is focused on the research context, and not on the societal impact of research. However, whilst I do not draw specifically on this strand of STS, I am aware that there is no impermeable barrier between issues ‘internal’ and ‘external’ to science.

4.3.2 Technoscience & The ‘Turn to Technology’

A unifying development for STS occurred in the mid 1980’s. The presentation of evidence that society shapes technology, and technology in turn shapes society (MacKenzie & Wajcman, 1999), plus the demonstration that the sociology of technology could be studied using the tools and theories of SSK (Pinch & Bijker, 2016) unified the strands depicted in Figure 3 around a focus on technology and materiality, and the society-technology interaction whereby the socio-technical and natural/material co-produce each other. This ‘turn to technology’ (Woolgar, 1991) continues today (Jasanoff, 2012), with particular focus on biomedicine (such as Carl May, Annemarie Mol, Nelly Oudshoorn, and Andrew Webster), ecology (such as Bruno Latour, Sheila Jasanoff, Matthias Gross, S. Lochlann Jain, and Jens Lachmund), and climate science.

As a result, the close study and description of knowledge construction is much less common within STS than before this turn;

> For me it’s not an “either/or,” it’s a “both/and,” but I still think that putting ‘society’ in there [In STS as “Science, Technology and Society] underlines what to me is the ultimate raison d’être of the field: it’s not simply a re-description of science in our discipline’s specialist language, it’s also the vehicle through which we reflect on what it means to be rational societies, manufacturing societies, or inventive societies.
>
> Pickersgill & Jasanoff, 2018: p321-322

One possibility for this thesis would be to draw on the post-1980 Science, Technology and Society framework, searching for technological manifestations of the particular ways expertise is accredited within, or diffuses across disciplines. Different disciplines may have varying conceptions of risk, or different connections to institutional and political structures of power, with consequence for knowledge-societies. These are interesting and important questions, but their answers partly depend on assumptions about the way disciplinary training acts to shape the design, conduct and interpretation of research activity. While the focus of the field has shifted away from knowledge construction, this is not because the puzzle of knowledge construction has been fully resolved. There remain 'vast spheres' about which 'next to nothing' is known (Schyfter & MacKenzie, 2018: p.345)

I consider population health one such sphere, and it is for this reason that I draw most heavily upon the study of science as exemplified by STS scholarship on the right hand side of Figure 3, concerned specifically with research activity and the construction of knowledge. This includes early work concerned with knowledge construction now associated with the technoscience movement (e.g., Laboratory Life).

As the conceptual foundation for these three strands (Strong Programme SSK, Ethnographic Studies, and Cultural studies of science) was laid by Thomas Kuhn, it is also necessary to revisit Kuhn and ask what contribution his work might make to a 21st century study of disciplines.

2.4.4 Revisiting Thomas Kuhn

Kuhn's concepts are frequently cited in the medical and public health literature, and *Structure* garners 500-700 citations each year in PubMed. However, only rarely are these ideas engaged with in a deep or substantial way. Many papers referencing Kuhn do so in their concluding paragraph, calling for a "paradigm shift". Three illustrative examples are included below:

> "…we recommend the adoption of a radically new cooperation paradigm." (Chang & Fraser, 2017)
>
> "This discussion highlights the need for **a paradigm shift**" (Bagozzi, 2007)
>
> "[We] recommend that it is time for a **paradigm shift** ." (Nowak, 2011)

This usage of 'paradigm' suggests that the term has evolved away from Kuhn's original meaning. In the Kuhnian sense, paradigm shifts cannot be 'recommended', they are the unavoidable and highly disruptive conclusion of scientific revolutions, which only occur once it has become uncontroversial (within a scientific community) to state that dominant tools and theories do not adequately explain or represent reality. Very generally, science is not in the habit of discarding established principles, perhaps because the wholesale re-tooling required to achieve a paradigm shift exacts an enormous intellectual and energetic cost on scientific communities (Kuhn, 1970). As such, the structure of most scientific activity is biased against revolutionary processes.

The above calls for paradigm shifts gloss over what must be overcome in order to *start* the requisite revolution. In Kuhnian terms, what must be overcome is normal science, a strongly disciplined pattern of practice which conditions the form and content of scientific knowledge. This thesis is concerned with disciplinary approaches to research conduct, and not with the nature of scientific progress *per se*. For this reason I leave aside the mechanics of scientific revolutions and paradigm shifts, and focus on Kuhn's foundational concepts of normal science, the paradigm, and disciplinary matrix.

2.4.4.1 Paradigm

In *Structure*, Kuhn argues that, above all, normal science (defined below) is supported by a common set of puzzle-solving procedures employed as models or pedagogic examples. This set of procedures derives from a common tradition of exemplary past achievements, or '*Paradigm'*.

This framing of the paradigm as a tradition of 'exemplary past achievement' was not made clear in *Structure*'s first edition. In the postscript to the second edition, Kuhn (1970) regrets causing "gratuitous difficulties and misunderstandings", by having used the term in two distinct ways:

> "On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. The first sense of the term, call it the sociological [...] [the second sense,] paradigms as exemplary past achievements." Kuhn, 1970: p.175

Kuhn finds the second usage deeper (philosophically), and makes clear that his model of scientific revolution rests primarily on paradigm-as-past-achievement (rather than paradigm-as-constellation of methods or beliefs). Confusion notwithstanding, Kuhn's dual uses of the term do not conflict, because the constellation of beliefs and techniques scientists hold in common *originates from* the tradition of scientific excellence which they, as a group, uphold and seek to emulate. Figure 5, below, contains an illustrative example I have assembled from the discipline of epidemiology.

When discussing a disciplinary paradigm, we might do so by laying out the values, beliefs, or preferred methods of a discipline, a selection of which appear in the top half of Figure 5 (labelled I). However, this is only a partial account. These particular study designs did not suddenly and spontaneously appear attractive to epidemiologists as productive endeavours. They derive their status as valid, reliable mechanisms for generating solutions to epidemiological problems from the historic, successful studies which pioneered their use. The methods are paradigmatic because of the exemplary past achievement with which they are connected (Labelled II in Figure 5). The circular arrows in Figure L4 reflect the way in which the two understandings of paradigm are self-amplifying: a method's continuing use reinforces the status of the original study as exemplary.

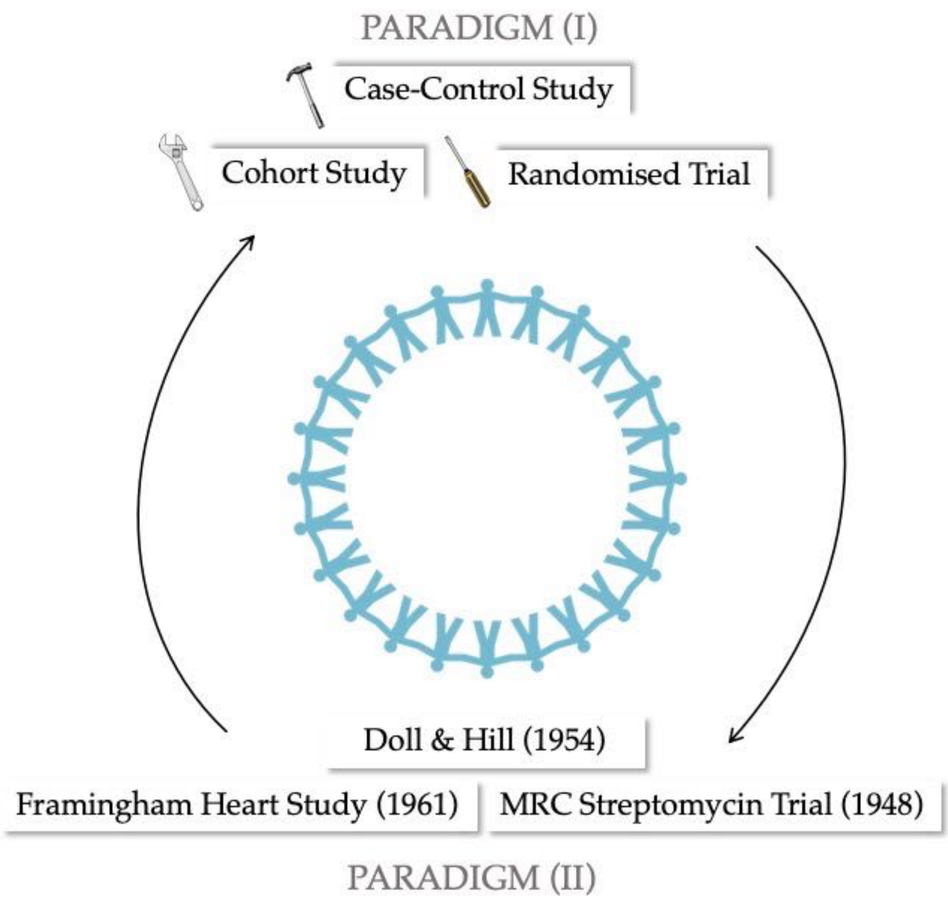

Figure 5. The Paradigm is the shared set of methods, beliefs and commitments (I), but also the tradition of excellence which established them as valid problem-solutions (II).

What of the human figures in Figure 5? Kuhn was clear that a paradigm is not a scientific community, and that disentangling the two is challenging, yet analytically important. A scientific community might be *identified* by its common paradigm, because “a paradigm is what members of a scientific community share… a scientific community consists of men [sic] who share a paradigm” *ibid* p.176. However, in unusual circumstances (a paradigm shift) a scientific community may discard its paradigm in favour of another. In such cases the scholarly community may endure, and outlast its paradigm. Such examples reinforce the ways in which paradigms do not float in the ether, sustaining themselves, paradigms endure via the action of living scholars, the continued application of paradigmatic methods, and the continued utterance of norms and beliefs connected to the discipline’s originating puzzles.

Paradigms and Disciplines

The word “paradigm” appears in *Structure* over 500 times, and “discipline” only 11 times. Kuhn’s articulation of the paradigm finds its most obvious application within disciplines, but the terms are not synonymous. Disciplinary communities almost always possess a paradigm, and Kuhn argues that the development of a paradigm is an essential step in the development of a discipline, because:

> “In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar.” (Kuhn, 1962:p15)

The paradigm therefore provides a framework for fact-gathering. For this reason, when a group of scientists adopt a common paradigm, the trappings of disciplinarity often follow. The group initiates some forum to share results (a journal), begins to schedule meetings and conferences, and to carve out institutional space by forming laboratories, departments and research centres. A discipline’s pre-paradigm phase is dominated by intense debate over disciplinary fundamentals, which can be all-consuming and exhausting, getting in the way of data collection and analysis. When a group of scientists coalesce around a paradigm (a shared set of problems, methods and concerns) such debates are put to rest, and the group can set off in the same intellectual direction, with agreed criteria for evaluating knowledge-claims. This thesis is concerned with disciplines in this second, stable phase.

Paradigms and Scientific Training

Disciplines are increasingly splintering into sub-fields (Weingart, 2012), each of which might be characterised by its own paradigm. In an era in which the disciplines are specialising at an exponential rate, and where scientists are increasingly accountable to publics outside academia, the relevance of overarching paradigms like “physics”, “economics” and “sociology” is contested (see Section 2.6), and something I investigate in this thesis.

Disciplinary splintering notwithstanding, scientific education generally retains a traditional mono-disciplinary structure (Weingart, 2014; Lyall, 2019; Evans, 1995), and Kuhn's account of scientific training suggests this should serve to sustain the influence of traditional disciplinary paradigms in research.

Kuhn argues that scientific education familiarises students with a pattern of investigative practice, which in turn shapes and determines which questions are considered acceptable, and the form acceptable answers should take. According to Kuhn, scientific training is dogmatic, a process of socialisation accompanied by "transformations of vision", allowing the student to inhabit the scientist's world, "see what the scientist sees and respond as the scientists does" (p.111).

Therefore, for Kuhn, scientific training results in a transformed worldview, because researchers under different paradigms have different "ways of seeing the world and practicing science in it" (Kuhn, 1970:p.4). By gaining familiarity with scientific paradigms, students develop a particular sense of what kinds of objects make up the world, and how these objects interact. These ideas

> "are firmly embedded in the educational initiation that prepares and licenses the student for professional practice. Because that education is both rigorous and rigid, these answers come to exert a deep hold on the scientific mind" (Kuhn, 1970:p5)

Kuhn's account of academic training is the thinnest and most weakly substantiated part of *Structure* (Barnes, 1982). It is supported by no empirical research, but neither is its target, the assumption that scientific training fosters creativity and open-mindedness toward non-paradigmatic approaches. Kuhn's view of scientific training is formed exclusively via case studies within the natural sciences, disciplines with clearly defined 'borders' and cohesive pictures of excellence. Within these fields (described in the next chapter as being 'strongly classified') training is more prescriptive and rigid than in 'weakly-classified' fields. I treat adherence to paradigm within less strongly-classified fields as an open question.

2.4.4.2 Normal science

The paradigm contains information about what constitutes a good question, good methods and good answers. In short: what good science looks like. Having grasped a paradigm, researchers can identify opportunities to apply paradigm-sanctioned methods to problems within their disciplinary domain. This is normal science, the quest for the familiar in the unfamiliar (Barnes, 1982) the routine swing of the disciplinary hammer and the mechanism by which researchers break nature into manageable, manuscript-sized pieces (Collyer, 2018).

If normal science is the search for opportunities to deploy paradigmatic methods, and most science is normal science, science might “seem an attempt to force nature into the preformed and relatively inflexible box that paradigm supplies” (Kuhn, 1970: pp24-25) . This is not to suggest a cookie cutter approach, as each study presents idiosyncrasies and analytical challenges. Nor is this a suggestion that normal science is *easy*. Finding the familiar within the unfamiliar can be extremely difficult, requiring creativity and intellectual flexibility. But the goal is nevertheless straightforward: to attack a new, unseen problem by identifying at least one key similarity with a previously solved problem or successful study. For this reason, Kuhn presents normal science as aiming not for novelty, or discontinuous discovery. Rather, normal science provides the next link in an established chain of enquiry:

> “In so far as he [sic] is engaged in normal science, the research worker is a solver of puzzles, not a tester of paradigms.” (Kuhn, 1970: p144)

Addressing the fundamentals of the discipline is not the business of normal science, because normal science “is predicated on the assumption that the scientific community knows what the world is like” (p5) and that *they agree* what the world is like. If Kuhn’s characterisation of normal science is accurate, there should be a clear difference in normal science as practiced in different disciplines. In addition, if normal science is underpinned by paradigm in the way Kuhn describes, the paradigm should be working (though, perhaps to varying degrees) to restrict the methods researchers apply, and also the kinds of questions researchers ask and answer. In this project I conducted qualitative interviews to explore normal science and the extent to which disciplinary paradigms appear to shape and restrict it in the study of health equity.

2.4.4.3 Incommensurability & Scientific Progress

> "A great deal depends on how far this concept accurately captures situations of theoretical rivalry in science" Benton & Craib (2010:p.30)

In a multi-disciplinary domain such as health equity, the question of what transpires when paradigms come into contact is important. Put simply, Kuhn argues that because paradigms contain their own standards for demonstration, and criteria for evaluating competing theories, there are no objective decision-procedures to justify the superiority of one paradigm over another. Sets of conventions cannot provide a basis for their own evaluation, and

> "proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve. Their standards or their definitions of science are not the same." Kuhn (1970:p.148)

The conclusion that paradigms are incommensurable also follows from Kuhn's description of scientific training, which provides scientists with a mode of perception, not simply a toolkit. The paradigm is a sense of what the world is like, and a template for conducting good science in it. From a Kuhnian standpoint, to prefer one paradigm over another is to do much more than prefer a set of methods, or a particular theory, it is to express a preference for a particular account of reality. For these reasons, Kuhn argues that scientists working under different paradigms cannot communicate fully, because if agreement is achieved on all important points, scientists are then working under a common paradigm. In practice, there are significant barriers to such agreement because

> "neither side will grant all the non-empirical assumptions that the other needs in order to make its case… they are bound partly to talk through each other" (Kuhn, 1970:p.148)

The doctrine of incommensurability precipitated an 'immense philosophical dog-fight' in the decades following *Structure's* publication (Hacking, 2012). However, both sides took positions more extreme than Kuhn himself . It is 'full' communication which Kuhn claims is restricted, and researchers 'partly' talk past one another. Kuhn also stressed that limitations on communication do not prevent researchers sharing and comparing technical results, and he '"do[es] not believe it [incommensurability] is ever total or beyond recourse" (Kuhn, 2000:p.124).

In this thesis I do not aim to confirm or refute Kuhn's account of paradigms as generally incommensurable. However, this aspect of the Kuhnian model casts doubt on the assumption that the epistemic task of interdisciplinary work is straightforward. In addition, where communication between disciplinary groups is consistently unproductive, it may be helpful to consider the extent to which the paradigms are commensurable.

2.4.4.4 The Disciplinary Matrix

Following a paper critical of Kuhn's plural usage of 'paradigm' in *Structure* (Masterman, 1970) Kuhn (1970: postscript) clarified that a scientific community is identifiable by its shared *disciplinary matrix*, a set of elements which includes accepted theoretical and empirical laws, beliefs, values and symbolic representations. This includes the "collective metaphysics of the group […] the entities and powers which appear in or are used to explain the laws" (Kuhn, 1967:Card 13). For Kuhn, the most important element within the matrix is the paradigm, the shared set of valid puzzle-solving procedures derived from a discipline's traditions of exemplary past achievement. In this thesis, a literal disciplinary matrix is assembled to guide analysis, laid out in the next chapter.

2.4.4.5 Criticism of Kuhn

Kuhn's writings continue to have a "profound effect" (Benton & Craib, 2010:p.58) on scholarly and public perception of scientific activity. *Structure* generated its own critical literature, and has attracted over 120,000 citations since publication in 1962, including whole books on the topic of its significance (e.g Barnes, 1982; Richards & Daston, 2016).

Initially, *Structure* met with significant critical reception, primarily among philosophers. Some lines of criticism resolved as Kuhn clarified his position, or changed his stance. As the critiques of Kuhn's work are vast, it is not possible for me to review them all. Rather, I focus on persisting avenues of criticism relating to my use of Kuhn; to the claim that paradigms cannot be neutrally evaluated, the validity of normal science, and the application of the Kuhnian model to science after 1945.

Kuhn's description of normal science was immediately criticised, most notably by Karl Popper, J.W.N Watkins, and Steven Toulmin. Popper (who viewed science as in constant revolution via bold statements which are vigorously tested) viewed Kuhnian normal science as *un*scientific, and found Kuhn's "normal" scientist a pathetic, poorly-educated creature (Popper, 1970). Popper also disputed Kuhn's claim that there is no neutral territory from which to evaluate competing paradigms.

Watkins charged that normal science describes science at its worst ('hack science') and is accurate only for periods of scientific stagnation, mere 'defensive metaphysics' (Watkins, 1970: p28). Both Watkins and Popper rejected the notion of distinct scientific communities unified by dogma, favouring instead a vision of science as a single, open community. Steven Toulmin asked whether the normal and revolutionary sciences are really specific modes distinguishable in the manner Kuhn suggests.

Kuhn responded to these critiques in a lengthy reply published alongside seven critical essays (from which the above quotations come) in "Criticism and the Growth of Knowledge" edited by Imre Lakatos and Alan Musgrave (Lakatos & Musgrave, 1970).

Neutral Evaluation of Paradigms

Against the charge that rationality and logic serve as neutral tools for the evaluation of paradigms, Kuhn clarifies his view that while scientists have good reasons for selecting one theory over another, what is missing is a neutral *language* to discuss the basis for such a decision. In a prelude to his later writings concerned with disciplinary lexica (discussed and applied in Chapter 10), Kuhn clarified his position as referring to the existence of an observational language shared in its entirety by two paradigmatic perspectives, which he denies. He argued that this stance is grounded in a view of language which 'fits the world', reflecting how the meaning arises and circulates within scientific language communities. Kuhn charges his critics from the Philosophy of Science as having demonstrable lack of interest and awareness of such real-world use of language, central to the function of paradigms, as Kuhn defines them.

Normal Science

Responding to criticism of normal science, Kuhn argues that if scientific revolutions exist (which Popper and Watkins did not dispute) some other scientific state must fill the gaps between them, and normal science is this other state. Kuhn also positions normal science as following in an uncomplicated way from Popper's own statement that theoretical frameworks are a prerequisite for research; "scientists necessarily develop their ideas within a definite theoretical framework" (Popper, 1970: p51). If revolution is the rejection of framework, then normal science is adherence to framework, something which all scientists do (not only plodding, uncritical minds). For Kuhn, Normal Science is not merely the absence of revolution, scientific frameworks 'must be lived with and explored before they can be broken' (Kuhn, 2000: p136) and this exploring and inhabiting comprises normal science.

Application to Science Post-1945

Science has changed radically since the 1960's when *Structure* was written, and is unrecognisable when contrasted with the 18th century activities which formed Kuhn's source material. Today, biotechnology rather than physics is the scene of rapid, controversial development, and the arrival of the computer and internet have transformed scholarly communication and dissemination. For these reasons, the straightforward application of Kuhn's account of science to 21st century research has been questioned (Hacking, 2012). Mario Biagioli's (2009) critique of the Kuhnian notion of paradigms as being 'too holistic' is illustrative of a common line of argument against Kuhn within science studies:

> The fast-paced rise, decline, and recombination of scientific disciplines and departments indicates that Kuhn's concept of paradigm is no longer descriptive of most current scientific practices [...] Kuhn's paradigm is simply too holistic a construct and puts too much emphasis on the intellectual and social cohesion of a group and on the uniformity of its training to match the remarkably diverse and mutating scenarios of contemporary research. (Biagioli, 2009:p.819)

In a similar vein, Galison (2016:p.66) charges that the paradigm is too 'monolithic' to be fruitfully applied to present-day research settings, characterising *Structure* as a

> Valiant and productive analysis of the physics of the 1930's, done in the 1940's about the science of the seventeenth, eighteenth, and nineteenth centuries.

The central argument is that due to the rapid pace of evolution within the modern academy, differences within disciplines now overwhelm differences between disciplines. This does not agree with my own experience in population health research, but if even if it did, the absence of cohesion within a discipline does not necessarily imply the absence of important distinctions between disciplines. And, where disciplines coexist in multidisciplinary domains, power imbalances between disciplines may mean that understanding such epistemological and cultural difference is key to understanding the field. Scholars focused on the history of a single discipline may naturally become finely attuned to the extent of disagreement within that discipline, and the notion of paradigm may be of limited use for the description of such heterogeneity. However, it may *also* be true that the paradigm captures something important about the differences between disciplines. In addition, the Kuhnian lens is much more than disciplines-as-paradigm (in Kuhn's writing, and in this thesis). Even if 'disciplines' are not describable with reference to a *single* paradigm, the Kuhnian model of scientific practice may still, fruitfully, apply. In "paradigms and exemplars meet biomedicine" (2016) Angela Creager notes that Kuhn's model works "surprisingly well" within her field, and that Kuhn's description of scientific training as "learning to see a problem as like a problem already encountered" (p.159) resonates, despite being written some 50 years prior, and based on scholarship from previous centuries. Even in a setting where methods progress rapidly, 21st century biomedical scientists do "reconfigure their local experimental systems to mimic the success achieved by others" (p.159).

Kuhn's account of how disciplinary training shapes perception, guides scientific practice and inhibits communication is powerful in its generality, but certainly lacking in fine-grained detail. Perhaps this "maddeningly malleable" (Creager, 2016: p.162) quality is a strength, allowing empirical detail to be added on top of Kuhn's original picture, painted as it was in broad-stokes. As was discussed in Section 2.4.3 (and visualised in Figure 3) such detail has been added gradually, by various scholars over several decades. These scholars headed in the direction suggested by *Structure,* and fleshed out mid 20th century generalities about 'science' with contemporary observations in numerous settings. When combined with Kuhn's own writing, these subsequent strands of work help to bring the Kuhnian model into the present, and create a richer picture of science as practiced today.

2.5 Interdisciplinarity

Just as the literature regarding 'disciplines' is fragmented, the literature concerning interdisciplinarity is also 'disjointed and dispersed' across a number of strands (Lyall, 2019:p.7). A complete review of the literature relating to interdisciplinarity is outside the scope of this thesis. However, the question of how apparent differences might impede researchers' efforts to collaborate is important. Indeed, during field work, many interviewees expressed the hope that the implications of my findings for the pursuit of interdisciplinary knowledge would be dealt with in this thesis. Chapter 10 addresses this question, and some specific literature supporting and contrasting with my findings is presented in that chapter. To provide a more general context, I now sketch an outline of the study of interdisciplinarity, and specify the terminology I employ.

2.5.1 Terminology

In analysis of interdisciplinary research, a variety of terms are available. Before turning to the set of terms used to discuss interdisciplinary work, I first note that interdisciplinary research is not always collaborative. That is, the integration of disciplinary perspectives can happen within the mind of a solitary scholar (Thompson-Klein, 2012). In a late essay, Kuhn seems to suggest this is the *only* way full communication across paradigmatic boundaries can happen, because communication from one disciplinary lexicon into another requires a 'bilingual' individual (Kuhn, 2000). I explore this possibility in Chapter 10.

Figure 6, below, is Alexander Jensenius' visualisation of the terms intra-, multi-, cross-, inter-, and trans-disciplinary research as defined by Zeigler (1990) and Stember (1991).

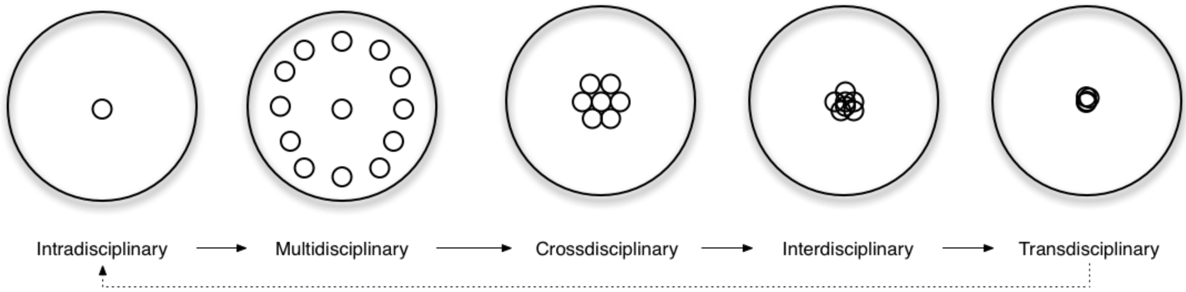

Figure 6, Reproduced from *Disciplinarities: Intra, Cross, Multi, Inter, Trans*. A.R Jensenius, 2012.

According to this framework, *intradisciplinary* work draws on a single disciplinary perspective. *Multidisciplinary* research involves more than one (mono)disciplinary perspective, but no attempt is made to synthesise approaches. In *cross-disciplinary* research, a problem located within one (central) discipline is addressed from the perspective of other disciplines.

Stember defines *Interdisciplinary* research as the “integration of disciplinary approaches bring[ing] interdependent parts of knowledge into harmonious relationships” (Stember, 1991:p.4). Therefore, simply involving members of different disciplines does not meet the standard implied by this definition, some integration of disciplinary perspectives is required. Finally, the term *transdisciplinary* applies to particularly successful instances of interdisciplinary integration, which result in the new, unified frameworks (perhaps giving rise to a stand-alone discipline, as per the dotted arrow in Figure 6)[2]. I employ the above definitions in my analysis, with an awareness that the term ‘interdisciplinarity’ is used in everyday contexts to refer in a rather more general way to any and all of the panels in Figure 6 (Frodeman, 2017). Indeed, this is how my interviewees seemed to use the term.

2.5.2 The Promise and Challenge of Interdisciplinary Research

Interdisciplinary research has become a central, load-bearing plank of research policy internationally (Bammer, 2013), but also presents a significant organisational problem for universities (Weingart, 2014). The practical obstacles to interdisciplinary careers are well-documented (Lyall, 2019) and include the low valuation of interdisciplinarity in academia, difficulties associated with publishing interdisciplinary research, and the lack of funding and career opportunities outside disciplined trajectories (LERU, 2017). These challenges originate from the structures and norms of academic organisations (Lyall & Fletcher, 2013) which strongly favour *intra*disciplinary career progression.

The disconnect between the exciting idea and challenging reality of interdisciplinary research has been written about for over 20 years (Thompson-Klein, 1996) and the importance of disciplined structures within universities appears to persist 'unfettered' (Weingart, 2012). In interviewing interdisciplinary Early Career Researchers (ECRs) and University administrators in the UK, Lyall (2019) identified ‘a manifest

[2] An alternative, common definition of “transdisciplinary” refers to research that includes non-academic partners as co-creators.

misalignment' between the strategic pronouncements regarding interdisciplinarity and the practical challenges of forging a career as an interdisciplinary scholar. Cutting through stated enthusiasm for interdisciplinary research was

> "the mantra of disciplinary excellence that characterises research-led universities. Despite the prevalence of the interdisciplinary rhetoric within their institutions and from their funders […] the ethos within their universities is one where the hegemony of disciplines triumphs, and interdisciplinarity still risks being seen as 'too soft for real tough minds' " Lyall, 2019: p.91 (quotation from Weirngart, 2000: p.29)

Lyall's study suggests that disciplinary (institutional) structures meaningfully shape research, and research careers. Additionally, these structures serve as formidable barriers to cross- multi- and interdisciplinary collaboration. Several studies of interdisciplinary cited in this section (including Lyall's) include health-related researchers, and note that interdisciplinary research is especially challenging for researchers straddling the social-science/medicine divide. However, I am not aware of any thoroughgoing account exploring these differences as connected to the content of disciplinary knowledge about population health, and/or explaining *why* it is so difficult for these disciplinary epistemologies to fruitfully coexist within a department, or project team. This thesis will contribute to this evidence gap.

2.6 Against Disciplines

In apparent contrast to the literature just presented, disciplinary structures and norms have been positioned as relatively unimportant features within the academic landscape, as relics of a simpler time and simpler science. While few authors make this claim, dedicated scholarship about disciplines is unusual, and so this kind of statement does represent a sizable proportion of the discourse surrounding academic disciplines. As was discussed in Section 2.4.2, non-disciplinary factors are presented as having eclipsed disciplinary norms in terms of influence on research output (Brew &

Manathunga, 2012). Several models of 'new' science have been proposed (Hessels & van Lente, 2008) and in this final section I discuss two well-known models which make specific claims regarding the dwindling influence of disciplines.

Mode-2 Knowledge: Gibbons 1994/ Nowotny et al. 2003

Gibbons and colleagues (1994) presented a picture of Mode 2 knowledge production, subsequently clarified in 2003 (Nowotny et al., 2003). Mode 1 Knowledge Production, "an internally-driven taxonomy of disciplines" (p.179), is framed as the 'Old Paradigm' of scientific discovery, now superseded "by a new paradigm of knowledge production (Mode 2) which is socially distributed, application-oriented, trans-disciplinary, and subject to multiple accountabilities" (p.179).

Rather than originating within disciplinary paradigms, Mode 2 knowledge is generated and evaluated within the context of its application. Gibbons and colleagues (1994) further characterise Mode 2 knowledge as being transdisciplinary, integrating a range of theoretical perspectives and practical methodologies "not necessarily derived from pre-existing disciplines" (Nowotny et al., 2003: p.186).

Under Mode 2 knowledge production, research is valued according to the extent to which it applies to and solves real-world problems. Under this model, criteria which determine research quality are not established by disciplinary communities, rather, they are defined outside the academy and are social, political and economic. Nowotny and colleagues argue three trends have driven this change:

I) The steering of research priorities by funding agencies

II) The increasing commercialisation of research

III) Science being increasingly accountable to a variety of publics

The three trends identified as driving Mode 2 science (especially the steering of research priorities by funding agencies) have already been documented as impacting HIDR within the UK (Garthwaite et al., 2016; Smith, 2010). However, no data are provided by Gibbons and colleagues (1994) to support the claim that scientists are more responsive to external notions of research quality than internal (i.e disciplinary) standards. Nowotny and colleagues (2003) are careful to position their model as a ‘reflective essay’ and not an empirical study, however the widespread existence of Mode 2 knowledge-producing communities “has not been supported by theoretical considerations or by systematic empirical evidence” (Weingart, 2012).

Post-Normal Science: Funtowicz and Ravetz 1993

‘Post-Normal’ Science is not a model of science as such, but rather the *kind of science* felt necessary to tackle a class of problems which, Funtowicz and Ravetz argue, are not amenable to solutions via Kuhnian normal science. Post-normal problems are characterised by urgency, uncertain facts, disputes regarding values, and high stakes. In practice, enacting Post-Normal science requires the extension of the notion of scientific “peer” in two ways. First, multiple disciplines should be assumed to have bearing on the issue from the outset. Second, the wider community, including lay-actors are invited to participate, thereby democratising science.

While employing a combination of disciplinary lenses seems desirable, if the Kuhnian characterisation of science is correct there is no neutral territory from which to balance or evaluate diverse disciplinary approaches to an urgent, high stakes challenge. Similarly, if the Kuhnian view of scientific training is accurate, scientists are not well-prepared for the kind of science Funtowicz and Ravetz advocate, and it is to be expected that the implementation of Post-Normal science is extremely challenging in practice.

Finally, the ‘closure’ of scientific communication discussed at the beginning of this chapter (scientific communication, once written for general consumption, has pivoted toward specialised audiences) would be expected to significantly complicate the inclusion of members outside the discipline and outside science within the research process. This PhD project provides an opportunity to sketch in some detail the potential barriers to combining disciplines and extending the notion of scientific “peer” in the way Funtowicz and Ravetz suggest is required to tackle complex problems via Post-Normal science.

The description of Mode-2 and call for Post-Normal science appear to me to represent examples of discourse emerging from the empirical appraisal of science which minimises the potentially important role of disciplinary structures. I do not argue that disciplinary identity is the most important factor shaping current research practice, but rather, in this thesis, seek to highlight what is lost when disciplines are ignored or overlooked in discussions of general trends within science, and academia.

2.7 Conclusion

In this chapter I reviewed the multi-stranded literature surrounding the form and function of academic disciplines. The study of disciplines is complicated by five challenges: the difficulty of defining and identifying disciplines; the fragmented nature of the literature about disciplines; various approaches to the separation of the ‘cognitive’ and ‘social’ elements of disciplines; an unbalanced focus on physics and natural sciences; and important variation across academic contexts (I discussed research, teaching, and practice). To navigate these challenges, I adopt Trowler’s (2014) view of disciplines as exhibiting strong family resemblance across contexts, whilst also understanding that the structure and culture of individual departments and laboratories is contextually-dependent. The Sociology of Professions, Higher Education Research, and Science and Technology Studies were reviewed in detail, and key contributions highlighted from each strand. Within all three strands, studies focused on the research context appear to be increasingly unusual, however this does not reflect a complete or total understanding of the construction of scientific knowledge within disciplines.

To sharpen my focus on the research context, and on the construction of knowledge within that context, I turned in Section 4 to the three streams of

‘Nature of Science’ STS as practiced in the 1960’s-1990’s. The Kuhnian model of science (the common foundation of these streams) was also reviewed in detail. Much seems to depend on whether the Kuhnian description of disciplines as adherent to paradigm holds true, as it cannot simultaneously be the case that disciplines “don’t matter” (as in Section 4) and also that disciplinary structures, conventions and norms are the principle barrier to interdisciplinary scholarship (as in Section 5). In noting this conundrum I return to the plural character of disciplines: simultaneously obvious and invisible, apparently weak drivers in a complex academy but also powerful limiters of interconnectivity and integration between academy members.

In the next chapter, I outline the specific concepts and theories which guide my analysis of disciplines in this thesis, and explain the basis upon which such diverse material can be coherently combined.

Chapter 3: Research Methodology

3.1 Introduction

Chapters 4 and 5 will present the research design choices that supported my approach to answering the research question and analytical aims outlined in Chapter 1 (p.38). Before beginning that description, in this brief chapter I present the principles and presuppositions which informed my selection of these methods.

As was discussed in the previous chapter, focusing on Kuhn's originating contribution to STS allows ideas from multiple disciplinary strands of enquiry to be usefully synthesised, and simultaneously employed, as elements within what Kuhn termed the disciplinary matrix. However, synthesizing disparate literatures poses an epistemological hazard, risking incoherence.

Charles Bazerman described this precise difficulty in the introduction to his book on academic genres "Shaping Written Knowledge":

> "In order to understand what I needed from the sociology of science or the philosophy of science or the history of science I had to encounter them in the context of their own problematics. **To steal random parts of different engines leaves one with a junkpile, even if one can create the appearance of a coordinated assembly"**
> (Bazerman, 1988: p.9, emphasis added)

At the beginning of this PhD, even arriving at a convincingly 'coordinated assembly' seemed impossible. However, the picking and mixing from diverse traditions in this project is not random, or haphazard. My approach to the question of disciplinary difference is grounded in the Sociology of Scientific Knowledge (Shapin, 1995) and the methodological principles of an approach to SSK known as the Strong Programme (hereafter 'Strong Programme SSK'), outlined in the next section. This tradition emerged early in my reading as being appropriate for supporting the type of insight I hoped to produce in this thesis.

In particular, my aim is to glimpse the causes of disciplinary diversity, not simply to describe disciplinary difference. This demands a particular set of theories and concepts, drawn from prior scholarship which goes beyond the description of difference in science. To make clear what I mean by this distinction I very briefly present three examples.

Bazerman's "Shaping Written Knowledge" (1988) takes a linguistic approach to disciplinary diversity, and might have been a descriptive catalogue of disciplinary writing styles. However, Bazerman's aim was sociological, not textual. Rather than describing how use of language tends to vary across disciplines, Bazerman asks what such variation portends, and employs theory from STS and SSK to understand the ways scientific language signals diverse ways of knowing.

Similarly, in the Enigma of the Aerofoil (2011) David Bloor notes that 20^{th} century German and British scholars of aerodynamics arrive at different answers to the question of how aeroplanes fly. Rather than describing this difference, Bloor explores how and why this difference arose, and the ways in which scientific cultures preconditioned their members to respond to certain kinds of 'proof'.

In Epistemic Cultures (1999) Karin Knorr-Cetina notes that High Energy Physicists tend to work in a cooperative manner, while Molecular Biologists do not. In painting a picture of how this difference is connected to, and rooted within the content of the two sciences, Knorr-Cetina moves beyond description toward explaining the origin, function and consequence of this difference.

Of these three works, only one (Bloor's) is identified as an example of Strong Programme SSK. However, in moving beyond description toward explanation, all three adhere to the 'Causal' tenet of Strong Programme SSK, which I also adhere to in this thesis. The methodological tenets of Strong Programme SSK are presented in the next section.

3.2 Strong Programme SSK

The methodological tenets of Strong Programme SSK (Bloor, 1976) are:

> *Impartiality:* with respect to the 'correctness', 'rationality' or success of a particular knowledge claims. Claims which are 'wrong' are not more interesting (sociologically) than claims which are 'right'.
>
> *Causality:* Concerned with the conditions which bring about beliefs, or knowledge. This includes but is not limited to social causes.
>
> *Symmetry in explanation*: The same causes, including social causes, must explain both 'good' and 'bad' science, both 'true' and 'false' statements. There are not special causes of bad science.
>
> *Reflexivity:* Any explanations must apply to sociology.

The *Impartiality* tenet prescribes that work should aim at the outset to study all scientific practices, not only good, bad or improper practice. This is connected to a directive from historian of science Martin Rudwick that studies of science must include

> "not only star performers but also minor actors, and walk on parts" (Rudwick, 1985: p.14).

The tendency of empirical studies of science to focus on 'star performers' was raised and critiqued by Becher & Trowler (2001), who rejected the conceptualisation of science as a 'snake-like procession' driven by elite institutions. Similarly, in interdisciplinary studies, scholars are calling for attention to both 'loud' and 'soft' voices (Lindvig, 2019). In this thesis I made effort to include researchers at varied career stages, and with high and low public profiles (discussed in Chapters 4 and 5).

As was mentioned in the thesis introduction, existing research tends to point to disciplinary difference as a resource in explaining some other feature of science (language, professional culture, publication norms). Such treatment of disciplinary difference as an analytical resource rather than topic can lead to the uninspiring conclusion that disciplines are different 'because they are different'. Applying the *Causal* tenet of Strong Programme SSK permits a more satisfying interrogation of disciplinary difference, because noting *that* researchers from different backgrounds approach science differently is not sufficient, some answer is required regarding *why* researchers write, hire, teach and publish differently. Said another way, rather than concluding that members of different disciplines appear to follow different rules, in following this principle I ask why the rules are being followed, and both how and why different disciplinary communities arrived at and sustain their diverse rule-sets (Bloor, 2011).

I view the causal tenet of Strong Programme SSK as helping to generate conclusions which are useful to researchers working within health equity scholarship, and population health. It is of little practical use to inform researchers that they experience difficulty in multi- and inter- disciplinary settings because of 'disciplinary culture' (less useful still for economists, epidemiologists and medical doctors who may lack familiarity with what a reference to 'culture' might specifically indicate.) Therefore, I follow the causal tenet in order to maximise the specificity of my findings, with the expectation that their potential usefulness for researchers is also maximised[3].

The *symmetry* tenet has been the subject of much comment and debate (Bloor, 2011). A common misconception is that in order to practice Strong Programme SSK scholars must abandon their understanding of the natural world, including all notions of 'true' and 'false', and adopt a view of reality as a social construction. Rather, what the symmetry principle requires is a willingness to entertain the view that the drivers of 'bad' science are the same as drivers of 'good' science, and that there are not special (i.e., social)

[3] Findings presented in Chapter 7 suggest this preference for useful findings may be rooted within my training in epidemiology.

causes of bad science absent in good science. This thesis is not specifically concerned with questions of true and false, or good and bad, but the symmetry tenet shapes my approach to the question of disciplinary difference in two ways.

First, rather than supposing that it is possible *a priori* to determine good research from bad research (or that there is a universal definition of 'good' science), the project has been purposefully designed to generate data regarding the type of research individual scholars view as ‘good’ or bad’ (see Chapter 5). Secondly, these data are viewed as a window into fundamental epistemological commitments, not as a test of who understands what kind of research is *really* ‘good’ or ‘bad’. This allowance for multiple perspectives draws upon the established notion within SSK that appropriate scientific conduct, “is tied to place and purpose” (Shapin, 1995: p.304), and that being 'right' and 'wrong' in science is frequently more complicated than initially appears to be the case (Latour & Woolgar, 1986; Kuhn, 2000).

Finally, the thesis adheres to the *Reflexivity* tenet by including sociology among the disciplines analysed.

3.3 Strong Programme SSK and Thomas Kuhn

It is common (e.g., Hacking, 2012) to dismiss the Strong Programme with reference to Kuhn’s 1991 Lecture “The trouble with the historical philosophy of science” wherein Kuhn identifies Strong Programme SSK as anti-science:

> “The most extreme form of the movement, called by its proponents “the Strong Program”, has been widely understood as claiming that power and interest are all there are. Nature itself, whatever that may be, has seemed to have no part in the development of beliefs about it […] I am among those who’ve found the claims of the strong program absurd: an example of deconstruction gone mad” (Kuhn, 1991: p.110)

This seems difficult to square with Kuhn’s own emphatic insistence that there can be no ‘objective’ discussion of nature. In any case, Strong

Programme SSK does not ignore the natural world, or reduce science to power and interest:

> "those who follow the Strong Programme do not treat the social world as something to which scientists respond instead of responding to the Natural World. The cultures, institutions, and interests that I have identified did not block the active involvement with material reality, but were a vehicle of that involvement, and gave a specific meaning to it." (Bloor, 2011:p.402)

Read closely, the tenets of the Strong Programme do not conflict with a Kuhnian model of science. In particular, *Structure* seems to precisely anticipate the Symmetry condition:

> "...if these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge." Kuhn, 1962: p.2

I perceive no conflict between the Kuhnian model of science and Strong Programme SSK. Major empirical landmarks of nature-of-knowledge STS (Knorr-Cetina, 1999; Latour & Woolgar, 1986) have drawn upon Kuhn whilst also adhering to the strong programme's methodological tenets (Latour & Woolgar specifically describe an aim 'to pursue the strong programme', p:152) In addition, Bartley (1990), a health equity researcher, has called specifically for more widespread attention to knowledge construction, via application of the Strong Programme in health-related sociology.

3.4 Populating the Disciplinary matrix

My use of Kuhn's ideas does not depend on the resolution of the longstanding arguments surrounding his work. I do not attempt resolve debate surrounding the nature of truth in science, or attempt to provide a general model of scientific progress. Kuhn's concept of incommensurability is relevant, but assumptions about the incommensurable nature of paradigms do not underpin my analysis, rather I seek to explore the extent to which disciplinary perspectives appear commensurable in HIDR. This PhD project is further motivated by an awareness that the Kuhnian model (developed as it was upon centuries-old physicists) may be of limited use in understanding 21st century research about health. A model of science built upon paradigmatic change may prove to be irrelevant to the case of HIDR where multiple (established) paradigms simultaneously coexist.

Nevertheless, the concept of the disciplinary matrix provides a helpful scaffold within which to hang specific dimensions along which (disciplinary) normal science might vary. The chapter concludes with a description of the concrete disciplinary matrix which guided my analysis.

3.4.1 Extant Disciplinary Taxonomies

3.4.1.1 The Biglan Taxonomy (1973)

Biglan (1973) noted a rise in scholarly interest in the organisation of university departments, but also the curious neglect of differences in the nature of research across disciplines. To get at these differences, 168 staff members at the University of Illinois were posted 30 pieces of paper bearing the names of various fields, and asked to staple disciplines which they judged to belong to 'the same category'. After the sorting task, participants were asked to rate each field on a set of bipolar dimensions: a) Pure/Applied, b) Physical/Non-Physical, c)Biological/Non-Biological d)"Of interest to me"/"of no interest to me" e)"Traditional"/"Non-Traditional" and f) life/non-life.

Biglan then analysed the resultant groupings, and arrived at three dimensions which he judged to reflect fundamental differences in the content of academic disciplines. These were:

- Life /Non-life: Whether or not a science concerns living organisms
- Pure/Applied: Concern (or not) with application to practical problems.
- Hard/Soft: Discussed Below

The Hard/Soft dimension was Biglan's own summary of emergent findings, and, while awareness of Biglan's paper is not widespread, the terms "Hard" and "Soft" have endured. Crucially, however, much usage of these terms does not reflect Biglan's intended meaning. By "hard science" Biglan did not mean concerned with concrete facts, rather, he meant strongly governed by a paradigm (and cited *Structure* in his discussion). By "soft", Biglan referred to disciplines within which content and methods are less uniform. Biglan subsequently abandoned the Life/Non-Life dimension, resulting in a 2 x 2 classification scheme for disciplines along the Hard-Soft and Pure-Applied dimensions.

The stability of disciplinary positions within the 2 x 2 Biglan schematic has been repeatedly demonstrated (Smart & Elton, 1982; Stoecker, 1993). Studies have shown that hard/soft and pure/applied differences align with differences in other properties, including students' theories of knowledge (Paulsen & Wells, 1998); job stress (Barnes et al., 1998) and variation in training for academic leadership (Favero, 2006).

Simpson (2017) sought to empirically test the relevance of the Biglan schematic in the UK, by analysing the kinds of courses which tend to be delivered at the same university. Using data from 23,000 courses delivered at over 100 universities, Simpson's first two explanatory dimensions corresponded with the pure/applied and hard/soft dimensions[4].

The stability of disciplines' status as pure/ applied, or hard/ soft suggests that these attributes may meaningfully shape research practice, and I add these elements to the disciplinary matrix.

3.4.1.2 The Kolb Taxonomy (1981*)*

Educational psychologist David Kolb (1981) did not cite Kuhn in "*Learning styles and Disciplinary Differences*" but presented a distinctly Kuhnian view

[4] Simpson classified 51 disciplines common to both analyses with 89% and 95% agreement, respectively.

of disciplines as possessing “different patterns of power and authority, and differing criteria for attaining status […] A sense of historical continuity and in most cases a historical mission” (p.233). Kolb also echoed Kuhn by noting that there is no neutral standpoint from which to evaluate and compare disciplines.

Kolb’s intent was to focus on learning styles, and to demonstrate the way in which these distribute in a predictable manner across undergraduate majors. Kolb administered his Learning Style Inventory to 800 undergraduates, then to 32,000 graduate students and faculty. Results highlighted two dimensions: The Abstract/Concrete dimension (students were asked to rate how important proficiency in mathematics was within their discipline), and the Active/Reflective dimension (derived from investigation of the extent to which teaching staff engage in consulting).

These dimensions seem to echo the Biglan dimensions, and were combined by Becher (1987) to form a single taxonomy, however they are not truly analogous (Simpson, 2017) and some oddities in Kolb’s original paper cast doubt upon their usefulness. For example, Sociology’s closest disciplinary neighbours are found to be Chemistry, Physics and Mathematics, which seems curious. Physics emerges in Kolb’s data as the most abstract of all disciplines, suggesting that, in Kolb’s sample, command of mathematics was judged more important in physics than *in mathematics.* Finally, nursing is not a “profession” in Kolb’s analysis, it is considered a Social Science because nurses rarely engage in consulting activities. Simpson (2017) was unable to reconstruct the Kolb dimensions in his study. For these reasons I do not incorporate the Kolb dimensions into my matrix. However, despite these issues, and lack of empirical support, in his discussion Kolb (1981) highlighted three additional, specific ways disciplines might be expected to vary upon empirical scrutiny, and these have been supported in subsequent study.

First, Kolb notes apparent variation in *preference for simplicity or complexity*, subsequently discussed and investigated by others (Daston &

Galison, 1992: Bloor, 2011). Second, Kolb discusses a variation in the sense of what constitutes valid knowledge, variation in what he terms *knowledge structure.* Nearly 20 years later, working within the Cultural Studies strand of STS, Karin Knorr-Cetina presented a highly detailed picture of this kind of variation between two disciplines (Knorr-Cetina, 1999).

Finally, Kolb discussed the ways different fields approach the trade-off between workability (or 'usefulness') and intellectual coherence, a trade-off at the heart of Bloor's comparative study of engineers and mathematicians (Bloor, 2011).

These three dimensions are added to my disciplinary matrix.

3.4.2. Additional Concepts

3.4.2.1 Basil Bernstein: Disciplinary Classification Strength & Code

Linguist and sociologist of education Basil Bernstein made a significant contribution to the study of communication via his sociolinguistic theory of language codes. Bernstein presented two types of language codes, the 'restricted code' for use within contexts where speaker and listener share assumptions and understanding on the topic, and the 'elaborated code', for use where less is presumed to be held in common (Bernstein, 1971). Scientific communication within a discipline may therefore be understood as a restricted code.

In addition to this conceptual framing of communication, Bernstein considered the nature of educational knowledge and curriculum (Robertson, 2003). Two concepts from this strand of enquiry have implications outside the classroom setting, and represent dimensions along which all disciplines might vary (Becher, 2001)

Strong & Weak Classification

Disciplinary classification relates to the strength of borders and boundaries between categories, and the extent to which categories (or other parts within a knowledge-system) are insulated from one another. Strong boundaries require maintenance, and the effort expended on such maintenance suggests well-maintained boundaries perform some valuable function within strongly-classified fields. Bernstein tied this characteristic to power relations in educational settings, arguing that power relations create, legitimise and reproduce boundaries between categories or groups within knowledge systems (Robertson, 2003).

Strong classification may be accompanied by strong 'framing' (Bernstein, 1996: p.27) clear delineation regarding the locus of control (i.e who has control over the selection and evaluation of sanctioned content). The combination of strong classification and strong framing in a discipline has been presented as helping to explain varying power dynamics across disciplinary cultures (Becher, 2001).

Similar ideas are present elsewhere in the literature. Biglan's "hard" and "soft" dimensions capture the extent of agreement among members about acceptable problems and methods, what Kuhn might describe as the flexibility or rigidity of a discipline's paradigm. These ideas connect with Bernstein's theory of classification, as 'strongly classified' disciplines are bound together tightly with a common core of methods and topics, while 'loosely classified' disciplines are not.

Of the available terms, Bernstein's discussion of classification does not depend on comprehension of underlying theory (as Kuhn's does) and does not have normative connotations in the way Biglan's Hard and Soft labels do. Therefore, in this thesis I refer to strongly and weakly classified disciplines, and Classification is added to the disciplinary matrix.

Cumulative & Integrative codes

Together, classification and framing strength determine a field's 'code', a regulative principle, tacitly acquired, which selects and integrates:

a) relevant meanings
b) forms of their realisation
c) evoking contexts (Bernstein, 1996: p.14)

The collection (or 'cumulative') code is characterised by strong classification, and sometimes by strong framing. The integration code is characterised by weak classification, and sometimes weak framing. Whether research activity is conceptualised as the *collection* of knowledge, versus the *integration* of knowledge into some whole, therefore represents another dimension along which disciplines might vary.

Becher & Trowler (2001) draw on Bernstein and discuss disciplines as having either "strong" or "weak" collection codes (pp.37-38), slightly distorting the original concept. Becher (1994), while not citing Bernstein or specifically mentioning codes, appears to connect this same idea to the nature of knowledge in disciplined research. In fields with a collection code, knowledge is described as cumulative and atomistic, accumulating as in a crystalline structure, advancing a frontier. In fields governed by integrative codes, knowledge is described as reiterative and holistic, being integrated and combined with existing knowledge, tending to re-address a set of fundamental questions. It is this elaboration of 'code' in disciplined science which I add to my disciplinary matrix.

3.4.2.2 Disciplines' Generative Power

I further anticipate that disciplines display vary in what Trowler (2014) terms their generative power; the power of disciplinary structures and cultures to affect other phenomena. In research, such power might manifest via success in obtaining funding, influencing policy, or the extent to which a discipline shapes academic practice in other disciplines (a possibility not anticipated by Kuhn's model).

This power does not accompany disciplinary labels in all cases, and may not be present in every instance, and is therefore best understood as variable and contextually contingent (Trowler, 2014).

3.4.2.3 Virtues and Transgressions

Two final elements emerge from consideration of disciplinary paradigms (which, as a reminder, I take to mean the set of problem-solving strategies rooted within a tradition of previous achievement).

When responding to queries regarding how scientific paradigms might be analytically accessed in practice, and within contemporary science, Kuhn responded:

> "Whatever scientific progress may be, we must account for it by examining the scientific group, discovering what it values, what it tolerates, what it disdains." (Kuhn, 2000:p.131)

Therefore, features of research valued highly within disciplines (what I term 'virtues') and features disdained (what I term 'transgressions') are added to the matrix, as these are likely to signal fundamental epistemological commitments.

3.4.3 A Concrete Disciplinary Matrix

The concepts presented in the previous section can be conceptualised as elements within a disciplinary matrix, depicted in Table 3, Below.

Paradigm (exemplary past achievement)
Pure/Applied Focus
Classification
Collection Code
Preference for simplicity/complexity
Knowledge Structure
Preference for usefulness / coherence
Cumulative Code / Integrative Code
Generative Power
Key Virtues
Key Transgressions

Table 3, Elements within the disciplinary matrix informing analysis in this thesis

The rows of Table 3 represent the dimensions along which, at the beginning of data analysis, I anticipated that disciplines may vary.[5] I treat the question of their specific relevance to health equity as open, to be explored in the results.

The following two chapters present the specific research methods employed, bibliometric analysis (Chapter 4) and semi-structured qualitative interviews (Chapter 5).

[5] The rows in Table 3 are visually connected to their supporting literature in Appendix B.

Chapter 4: Bibliometric Analysis Methods

4.1 Introduction

To make any comment on the disciplinary backgrounds present within health inequalities and disparities research (HIDR), and to sample appropriately for my interviews, reliable data describing the disciplinary makeup of the area is required. Bibliometric analysis is the application of statistical methods to data describing scientific texts and the citations among them (Gmür, 2003; Sugimoto & Larivière, 2018). I employed bibliometric analysis to map the intellectual territory "Research about Health inequalities and disparities" and to algorithmically subdivide the field into communities on the basis of citation patterns. Concrete outputs of the analysis included a list of the 250 most-connected authors working within HIDR, and a network representation of these authors. Next, a data collection process (described in Section 4.4) identified the disciplinary backgrounds of these 250 researchers, who became the population from which interviewees were sampled.

The employment of bibliometric analysis in this thesis had three purposes; to delineate the field of HIDR and map its disciplinary topography, to generate data describing the disciplinary mix of authors, and also to serve as the sampling frame for interviews. A detailed description of the method now follows.

4.2 Bibliometric Analysis

Bibliometrics is a branch of scientometrics, the "quantitative study of science, communication in science, and science policy" (Hess, 1997: p.75). Like history of science, philosophy of science and sociology of scientific knowledge, scientometrics turns the tools of science upon science itself (Leydesdorff, 2001).

Scientometrics tends to distinguish sharply between research 'inputs' (funding, technology) and 'outputs' (publications, patents) (Sugimoto & Larivière, 2018). Data regarding research input tends to be locally managed, and comparison across countries or disciplines is hampered by variation in methods and context. The social sciences and humanities are very rarely included in surveys of research inputs (Sugimoto & Larivière, 2018), as correspondence between inputs and outputs for these disciplines is not straightforward. Collection of detailed data describing scientific inputs also tends to be obtrusive, manual, and time consuming. Conversely, data regarding research output is widely available, and less variable between scientific and geographical contexts. Data can be collected unobtrusively, and collated into large databases. Therefore,

> "Despite issues in coverage (e.g., by discipline, language, and country), citation databases have become the standard for measuring research, primarily using the tools of bibliometrics […] They can be used both to complement other studies of science as well as to provide additional understanding of how knowledge is produced over time, across disciplines, and across the globe " (Sugimoto & Larivière, 2018: pp2-3)

Bibliometric methods are the set of statistical techniques used to analyse datasets which describe scientific outputs. By a variety of means (see Section 4.3.4) data relating to individual articles, authors, institutions and countries can be analysed, and mapped (Rip, 1988). The method has various strengths and weaknesses, discussed in the following section.

4.2.1 Advantages and Limitations

Advantages

HIDR is vast, and diverse, meaning the most active researcher could not have a birds-eye appreciation of the field. Bibliometric analysis provides an efficient and reproducible method for summarising data about published texts, and creating pictorial representations of a research area. Bibliometric methods can help to avoid distorted pictures of a field arising from researchers' local standpoint (Gagné et al., 2018). Additionally, "a bibliometric analysis covering an extended period of time can help us to pinpoint the most influential ideas/schools of thought (as proxied by associated authors) and the interrelationships among them." (Nerur et al., 2008: p.320). Bibliometric analysis is especially useful when the volume of relevant data exceeds what a human can read, and, even for small datasets, understanding bibliometric data without visualisation is challenging (Sugimoto & Larivière, 2018).

Limitations

Bibliometric analysis relies on repositories of citation data, often demonstrably incomplete and/or incorrect (De Battisti & Salini, 2013; Rafols & Meyer, 2007). The format of these data is also challenging (De Battisti & Salini, 2013) as bibliometric data are textual (lists of names and references), not numeric. Manipulating and cleaning text in large volumes is challenging because interacting manually with text is time consuming, and fraught, as a misplaced comma or semicolon can corrupt the data in some formats (as I discovered, see Section 4.3.2). Software can now receive data directly from repository databases (see Section 4.3.6) removing the necessity of interacting with data directly. However this introduces other difficulties, as feeding bibliometric data directly into an algorithmic process without cleaning or checking potentially provides misleading results (Sugimoto & Larivière, 2018).

Bibliometric analysis at the level of individual authors is especially challenging, as in addition to the challenges described above, names might be misspelled, middle initials are sometimes omitted, or multiple authors can have the same name. Future analyses will be greatly assisted by numeric author identifiers such as ORCID, but, for now, the cleanliness of bibliometric data is a significant issue, and peer-reviewed guidance for managing these difficulties in author-level analyses is limited. However, these problems, while challenging, are not catastrophic. The apparently random nature of author misspelling and misattribution allows this to be conceived as noise rather than bias, and the sheer volume of data which can be analysed helps overcome this noise. Nevertheless, some examples are not random (e.g. authors from certain countries are more likely than the rest of the field to share a surname). I emphasise that this project was conducted within the general limits of bibliometric data sources, and as such results are not definitive.

Beyond issues of data cleanliness, early-career researchers are disadvantaged by bibliometric methods, as time is required for citation to occur. Additionally, publications attract citations for a variety of reasons, and not all citations reflect approval or endorsement. The meaning of citation requires consideration; but a comprehensive theory of citation remains elusive (Leydesdorff, 2001). In this study, my aim is not to study the “best” HIDR researchers, but to identify and analyse the disciplinary backgrounds of the most-connected set of authors, as this provides a sense of the field’s structure, and organisation in citation-space.

Variation in citation patterns between disciplines (Fanelli & Glänzel, 2013) is an important consideration in this project. On average, social science articles cite more sources than natural sciences articles. This tendency could result in more links, and therefore a bias toward social scientists. However, there *are* more natural science articles, and average citation rates in the natural sciences are higher, perhaps balancing out their shorter reference lists. This is a complex issue and, regrettably, there is little peer-reviewed guidance on conducting bibliometric analyses at the level of authors rather than documents. The length of reference lists within my dataset is available for analysis, and could be investigated in detail, in future.

Finally, from a conceptual standpoint, ‘science’ cannot be reduced to the set of variables and indicators within bibliometric datasets (Leydesdorff & Milojević, 2012). The price of drastic simplification in science can be significant (Bloor, 2011) and careful attention to the trade-off between tractability and simplification is necessary. For this project, even given the limitations above, bibliometric analysis does represent the best method for scoping HIDR. However, this analysis, while aspiring to be an accurate picture, can only represent an abstraction built by traces left by certain scientific activities.

With these limitations in mind, the method is presented in detail.

4.3 Detailed Description of the method

Bibliometric analysis requires:

- A database describing relevant features of a collection of scientific articles, including references between them
- A search string to sample records from that database
- A unit of analysis, which form the ‘nodes’ of the network (e.g. individual authors, papers, countries, institutions)
- A method for calculating the appropriate ‘distance’ between nodes
- Agreed criteria for deciding which nodes should be included in the network (if the data contain more than is visually manageable)
- Software to take these data and produce a network
- An algorithm for detecting communities within the network

These elements are discussed in detail in the following paragraphs.

4.3.1 Data Source

The Web of Science (WoS) and Scopus are two, large bibliometric databases frequently used in bibliometric analysis projects, each with strengths and weaknesses (Sugimoto & Larivière, 2018). Both databases have a demonstrated bias toward journal articles compared to books (Mongeon & Paul-Hus, 2016). As articles are the principle method of result dissemination in the natural, engineering and biomedical sciences, this produces a bias toward these disciplines and away from the social sciences, arts, and humanities in bibliometric data (Norris & Oppenheim, 2007). As this project focusses on research about health (disseminated most-typically via peer-reviewed articles rather than book chapters) this bias seems acceptable.

Previous bibliometric studies of interdisciplinarity have drawn upon WoS data (Porter & Rafols, 2009; Rafols & Meyer, 2007). WoS imposes a more rigid data structure and has a superior journal classification schema (Wang & Waltman, 2016). For these technical reasons, WoS would be my preferred database.

However, Mongeon & Paul-Hus (2016) compared the active journals covered by Scopus (n= 20,346) and the WoS (n= 13,605) with Ulrich's index of 70,644 journals. Many of the almost 7,000 additional journals covered by Scopus are in the area of biomedical research, and so this difference is significant. Scopus also demonstrates superior coverage of the social sciences, and as HIDR is comprised of both biomedical and social scientists this difference is potentially significant for my study. Structural barriers to combining data sources necessitated the selection of a single database, and so on the basis of superior journal coverage, Scopus was selected.

4.3.2 Search String

The search string evolved as the project progressed, in consultation with my supervisors, and a librarian.

Initial analysis in 2017

The first iteration was completed in March 2017, quite early in the PhD. This early analysis utilised data from WoS, as I had not adequately researched the differences in coverage between databases. Similarly, the search string (below) was not thoughtfully developed (i.e. not developed via consultation with a librarian or my supervisors):

> "Health Inequalities" OR "Health inequality" OR "health equity" OR "social determinants of health" OR "Health disparities" NOT "cancer statistics"(title)

The exclusionary term was introduced in order to remove large, annual cancer incidence reports published in the US. These publications have a very large number of authors and attract many citations. At the time, I viewed this as a distortion of the network, and added the exclusionary term to reduce the influence of these atypical papers.

Repetition of the analysis in 2018

In 2018, a group in Barcelona published a bibliometric analysis of the health inequalities field spanning 1966-2016 (Cash-Gibson et al., 2018). The search string adopted by these authors seemed less restrictive and likely to include a higher proportion of relevant papers than my early effort. Having learned more about the superior coverage of Scopus, I took the opportunity to revisit and improve the analysis.

I reached out to the lead author of this publication to seek her advice regarding potential changes to my search string. She, my supervisors, and I agreed that the following change was desirable, as by searching for parts of phrases rather than entire phrases a larger number of relevant papers could be detected:

> (("health inequ*") OR ("health equal*") OR ("health equit*") OR ("health disparit*") OR ("social determin* of health"))

Rather than using an exclusionary term designed to act on a particular, small set of papers, the whole analysis was limited to papers containing fewer than 15 authors. In this way, papers with extremely long author lists *on all topics* would be excluded from analysis.

This decision meant that the bibliometric analysis needed to be repeated, along with demographic data collection for 50 new authors who appeared in the network following these changes. This process took approximately two months and delayed other parts of the PhD. However the additional work was felt to be worthwhile because the new search string included several thousand additional papers clearly related to health inequalities and disparities. Additionally, being new to bibliometric analysis, the opportunity to repeat the task with more understanding and experience was an opportunity to consolidate my skills.

Repetition of the analysis in early 2019

In early 2019 I discovered that one of the data files extracted from Scopus was unknowingly corrupted during data cleaning by a set of stray commas. Over 1,000 papers published in 2006 had been inadvertently omitted from both prior analyses. Additionally, I obtained funding to travel to Europe to meet with a leading scientomatrician in the Autumn of 2018, who, while stating that the existing analysis was ‘fine for your PhD’, felt additional cleaning and checking of author names would be expected by specialised peer-reviewers. When the above issue with the analysis was discovered I decided to implement the suggestion of further data cleaning, and repeated the analysis. As this operation would further increases the chance of data corruption, I decided that the eccentricities of bibliometric data required a more sophisticated approach to data management than the manual manipulation of 29,000 rows of text (i.e, editing a document 15,000,000 words long).

I therefore moved the data into a Structured Querying Language (SQL) database. A detailed discussion of this technology is outside the scope of this chapter, but SQL works similarly to a connected chain of Excel Spreadsheets, with rigid enforcement of rules which facilitate safe interaction with parts of the data at a time (e.g., papers by a particular author, or titles containing a particular word). Having to learn a new programming language and repeat the analysis was arduous, but worthwhile, as the finished network is clearly superior to the two which proceeded it (see Appendix C for previous iterations).

4.3.3 Unit of Analysis

Bibliometric analysis almost always occurs at the level of publications or documents (e.g. Merigó & Núñez, 2016; Soteriades & Falagas, 2006), including three previous studies of HIDR (Almeida-Filho et al., 2003; Bouchard et al., 2015; Cash-Gibson et al., 2018). These analyses take the document as the entity of interest, and links between documents are detected and visualised. As the aim of my study is to improve understanding of the disciplinary trainings which inform the conduct and content of HIDR, and it is authors who possess disciplinary trainings, authors are the objects of analysis in this study.

4.3.4 Method for calculating distance

The network visualisation is created by examining the strength of citation links between all pairs of authors within the source data. Several alternatives for calculating link strength between authors are available, the most common are visualised in Figure 7.

- *Co-Authorship analysis:* How often do each pair of authors co-author publications?

- *Bibliographic Coupling:* How often does each pair of authors reference a given common third author?

- *Co-citation:* How often are two authors cited within the same paper by a third author?

- *Direct Citation:* How often does a paper authored by one author cite another particular author?)

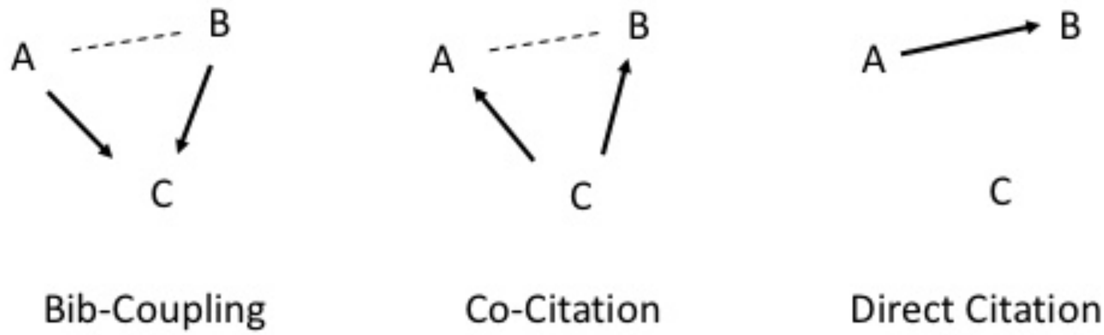

Figure 7, three ways to calculate the link between authors A and B. (Solid arrows represent citation links)

Co-authoring articles seemed a narrow and inappropriate measure, because this approach ignores information contained within citations, and researchers can only be linked if they have co-authored an article.

Co-citation and Bibliographic coupling are considered superior to direct citation for most applications (Boyack & Klavans, 2010; Fanelli & Glänzel, 2013) because they access information from a larger number of reference lists when calculating link strength. These methods are particularly suitable for analyses of small datasets, which may not contain a sufficient number of direct citation links to appropriately calculate distances between all pairs of authors, producing a 'sparse' network. My sample is quite large (29,212 papers), and so all three methods could conceivably be used.

I perceived two advantages of direct citation; It is the simplest method to explain and understand, an important factor as the audience for this analysis is not scientometric experts. Additionally, as the name suggests, direct citation *directly* reflects the ways in which citations (which stand variously for a kind of scientific currency or as proxies for ideas; Leydesdorff & Milojević, 2012) flow across a research area. For these reasons, I opted for direct citation, after confirming that my data were sufficiently large to avoid a sparse network.

4.3.5 Criteria for including nodes (authors)

HIDR is too vast to allow the visualisation of every active author. The pool of eligible authors was narrowed in two ways: First, authors needed to have at least five eligible documents (publications including relevant keywords in the title/abstract) to be included. This criterium excludes authors whose main body of work is outside HIDR. The inclusion of such “interlopers” within the project was a concern expressed by some interviewees (see Chapter 6) and so it proved important to be able to describe the sample of authors as being established within HIDR to this extent.

Second, the final network is not the entire, global HIDR network but the 250 most-connected set of authors. This means that “satellite” communities of researchers who cite familiar literature among themselves, but do not (ever) cite the HIDR ‘mainland’ are excluded. 250 was selected as the maximum size of the network as this was felt to be at the upper limit of what was logistically and visually manageable.

4.3.6 Software

I considered general network analysis tools Pajek (Batagelj & Mrvar, 2004) and Gephi (Bastian et al., 2009), as they have powerful analysis and visualisation capabilities, however the work required to prepare data output from Scopus into these programs is not trivial. Also considered were the specialised bibliometric analysis programs VOSviewer and CitNetExplorer (Van Eck & Waltman, 2014). VOSviewer was designed especially for bibliometric networks, has the shallowest learning curve and can receive data exported directly from Scopus and WoS. VOSviewer can produce a variety of bibliometric networks, and has a strong focus on visualisation, but also text-mining capabilities. CitNetExplorer visualises only direct citation networks and offers more powerful analysis capabilities than VOSviewer. CitNetExplorer can also receive output from Scopus and WoS directly, and permits the editing of data during analysis (very useful when names are spelled incorrectly or inconsistently).

For this project the additional power of general network analysis software was felt to be outweighed by the steeper learning curve and data conversion requirements. I chose VOSviewer for this analysis as it is very easy to use, does not require data conversion, supports the algorithm I wished to use (see below), produces visually appealing results, and can generate a variety of network types.

4.3.7 Algorithm for detecting communities

'Communities' (clusters of connected authors) can be detected within a network via clustering algorithm. Algorithms of this sort seek to place nodes (here, authors) into groups in order to maximise the modularity of the configuration. Modularity is a statistic which compares the performance of a clustering algorithm with a worst-case scenario baseline; a randomly generated set of groups (where 'performance' is the proportion of citations which do not cross cluster boundaries, explained below).

An example below contains 13 authors and 17 links:

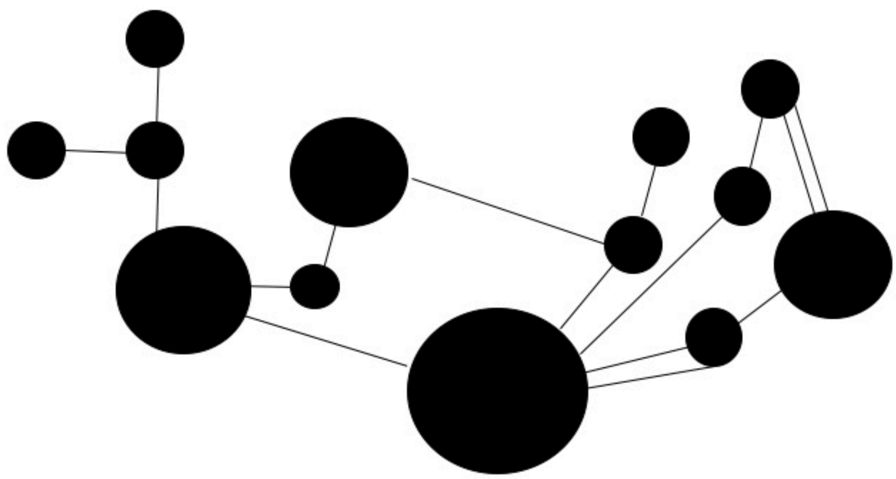

Figure 8, a sample network.

This network might be bisected (into two clusters) at the following 5 points:

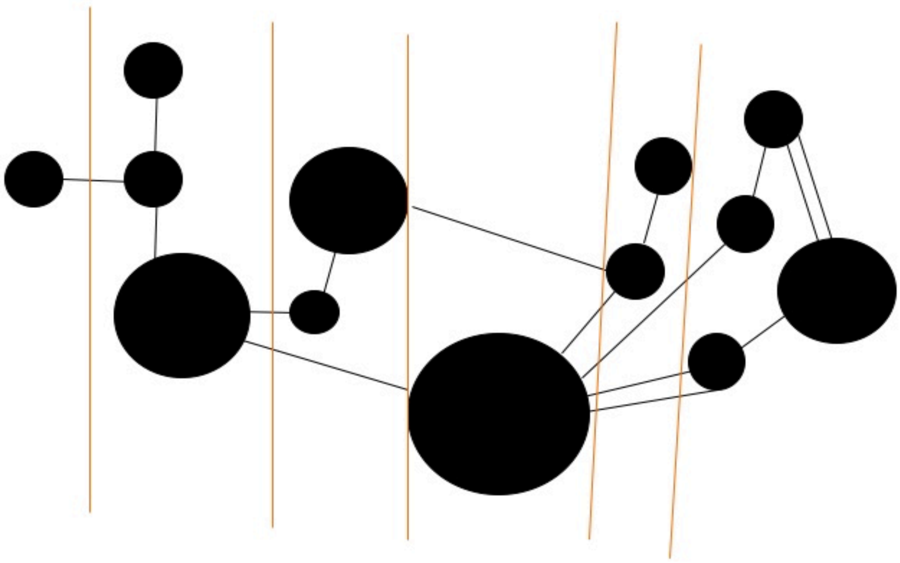

Figure 2a, 5 ways of dividing a sample network into two clusters.

The fourth location might be randomly selected, and the following groups constructed:

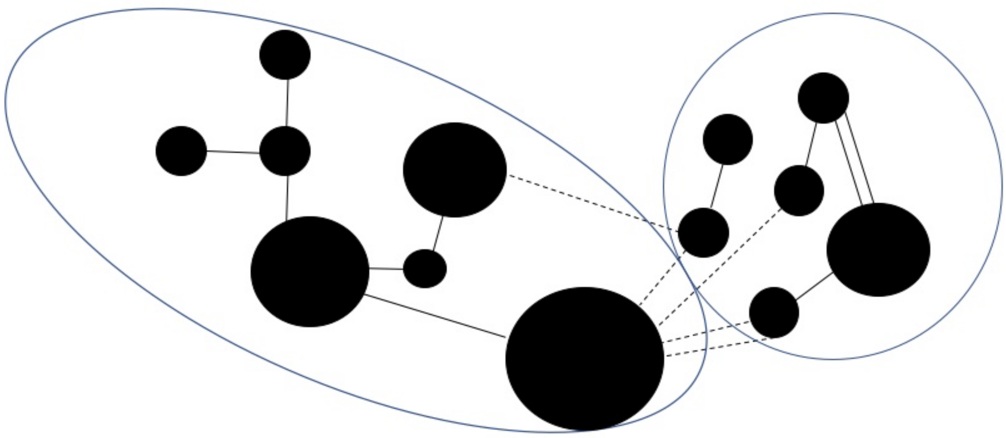

Figure 2b, calculating modularity for two clusters

With these random groups, 11/16 links (69%) are contained within the clusters, and 5/16 links (31%, dotted) cross the cluster boundary. Moving the large, central author to the right-hand cluster would result in the following change:

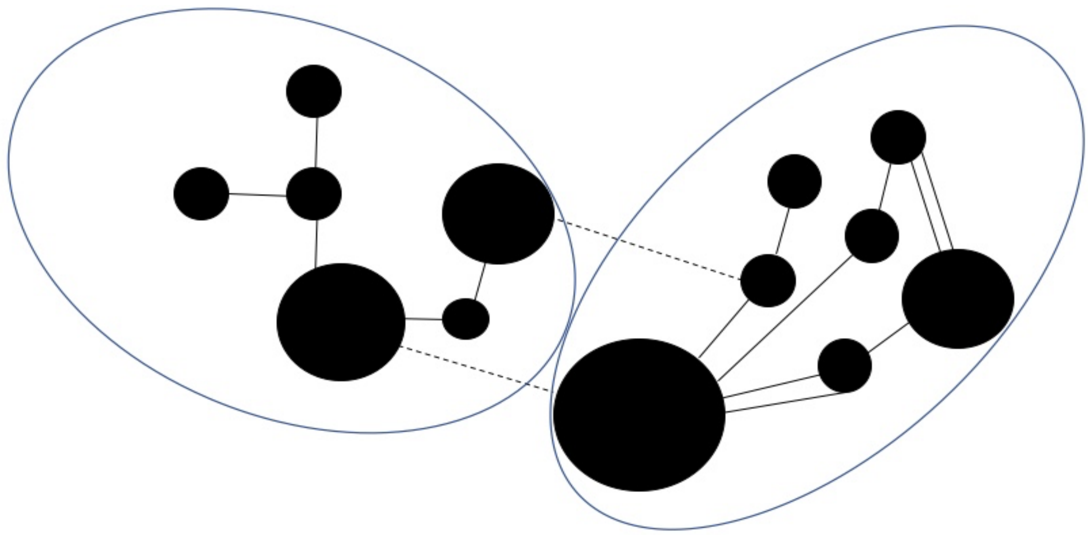

Figure 2c, Alternative clusters with higher modularity

Now 14/16 links (87.5%) are contained within clusters, and 2/16 (12.5%) cross the boundary. This configuration has a higher modularity and so would be preferred by a clustering algorithm.

Waltman and Van Eck's (2013) Smart Local Moving algorithm was used in this study. This algorithm initialises by assigning each author to a cluster of which they are the only member (i.e, begins with 250 clusters), and moves authors between clusters until no increase in modularity is achievable. This algorithm was selected as it can be implemented within VosViewer, and has been documented obtaining modularity values as high or higher than more popular local moving algorithms (Blondel et al., 2008; Rotta & Noack, 2011).

4.4 Collecting Author-Level Characteristics

Following identification of the 250 most-connected researchers, I collected demographic data for each author including the discipline of undergraduate and postgraduate training, doctoral qualification and country of residence. This information was collected from online sources (unobtrusively) for the vast majority of researchers. Where information was not publicly available (or conflicting information was available) I requested it via email. Complete data were collected for 89% of authors.

This detailed author-level dataset supports and augments the bibliometric analysis, which has access only to citation data and is blind to factors such as geography and discipline. The careful manner in which this data were collected (including confirming with authors in the event that data were ambiguous or missing) represents a major improvement over existing attempts to discuss disciplinary training via bibliometric methods, which typically rely on institutional affiliation or address data contained within the bibliometric dataset, notoriously messy and incomplete (D'Angelo et al., 2010).

4.5 Data Cleaning

Bibliometric analysis at the author level requires a high standard of data-hygiene. As the unit of aggregation (authors) is small, typographical errors can significantly distort results. Unfortunately, systems which ensure authors are identifiable across multiple publications have been only recently developed, and large datasets present many inconsistencies. For this reason, consideration and effort was devoted to the cleaning of the source data for the final analysis.

A large number of small problems presented early in analysis. Misspellings of author names, missing initials, the presence or absence of 'Jr' created a number of "authors" who were not really distinct individuals. These were quite easily dealt with via the 'thesaurus' function in VOSViewer, which permits the user to read a table of required replacements alongside source data. In total, 132 misspellings were identified by scrutinising the list of included authors, sorting by publication count and examining outliers.

4.5.1 Authors with the same name

The biggest problem encountered was the fact that multiple researchers had the same name and initial. Where this occurs, there is no basis upon which to discriminate between individuals.

This is a challenging problem to solve, as not only do names need to be changed within records relating to these authors' publications, every reference to each paper needs to be manually reviewed and adjusted. In this study, the following names represented more than one author in the Scopus source data: Chen, J., Kim, J., Lee, J., Lynch, J., and Jones L.

This difficulty has been acknowledged by previous authors (Wagstaff & Culyer, 2012; Jonnalagadda & Topham, 2010) but few workable solutions are available. D'Angelo (2010) proposed algorithms using address or institutional affiliations to 'normalise' authors, however inconsistencies in the ways these fields are reported, combined with the fact that authors regularly move institutions, or hold concurrent appointments complicates this approach. Without painstaking manual review (impractical for a sample this size) it is not possible to systematically identify *every* example where authors share a common name within the dataset. As a workable alternative, I adopted the approach of Wagstaff & Culyer (2012) and manually checked all names appearing within the network, updating source data as required. This means that it can be stated confidently that all authors within the final network visualisation are unique individuals. Over 800 manual changes were ultimately made to source data files.

4.6 Disciplinary diversity

The analysis detailed so far identifies the blend of disciplinary backgrounds present within HIDR, but provides no way to summarise that diversity. Stirling (2007) provided a framework for measuring and discussing diversity widely utilised in ecology and scientometrics. Stirling's three components of diversity are

1) Variety
2) Balance, and
3) Disparity

Variety describes how many options are represented within the dataset: For example, do researchers all come from the same single, two or ten disciplines? Balance describes the extent to which these different options are present. Are most researchers from a small number of disciplines (with perhaps a handful of exceptions), or more evenly distributed? Disparity reflects the extent to which the options present are different from one another. Have researchers trained within a neighbourhood of closely-related disciplines (medicine, epidemiology), or a set of more diverse disciplines (engineering, linguistics)? An ideal measure of diversity accounts for all three features.

The Shannon Index (Spellerberg & Fedor, 2003) captures the uncertainty of predicting from among a set of options, exploiting the fact that as diversity increases, uncertainty of prediction also increases. In my study, Shannon diversity reflects the uncertainty surrounding prediction regarding the discipline of a researcher's PhD (or other highest degree), within the network clusters. The more diverse the cluster, the greater the uncertainty about the discipline of a randomly selected researcher.

The Shannon index is unit-free, and is therefore difficult to directly interpret (i.e., a Shannon diversity of "2" has no natural interpretation). Furthermore, a doubling (or halving) of the number of categories/individuals in a dataset does not produce the expected, equivalent change in the Shannon index. Jost (2007) suggests taking the exponent of the index to produce a 'number equivalent' which has these desirable qualities. The number equivalent Shannon index is presented in my analysis, and can be interpreted as reflecting the number of (equally-balanced) disciplines represented by an author group[6].

The Shannon index accounts for variety and balance, but not disparity. The *integration index* (Rafols & Mayer, 2009) utilises a matrix of journal Subject Categories to incorporate information about how close or distant disciplines are (in citation space). I employed Rafols & Meyer's mapping in this project. To facilitate comparison with other studies using the integration measure, authors were classified into a subject category using the discipline

[6] E.g. A group of 100 authors with a Number Equivalent Shannon Index of 6 implies that there is diversity equivalent to 6 balanced disciplines present, meaning 6 disciplines of (100/6) ~17 authors each. A group of 100 authors with an index of 7 is more diverse, suggesting 7 disciplines of 100/7~14 authors each.

of their PhD or highest degree. Of the 244 Web of Science subject categories, 36 categories are represented by members of the final HIDR network.

The Subject Category “Public, Environmental and Occupational health” contains researchers trained in Epidemiology (n=39) , Public Health (n=17), Social Epidemiology (n=3), Health Promotion (n=2), Health Education (n=3) Health Behaviour (n=4), Environmental Health Science (n=2), and Community health (n=1). This subject category is perhaps too general for application within a health-specific field such as HIDR, and a future study might generate a set of health-specific subject categories and mappings. Nevertheless, the categories as originally published are adequate for comparing disciplinary diversity between the bibliometric clusters.

Chapter 5: Qualitative Interview Methods

In this chapter I describe the approach taken to sampling, conducting, coding and analysing interviews. I describe interview conduct and analysis, and discuss the challenges and mishaps arising along the way.

5.1 Sampling

Interviewees were selected from the 250 researchers included in the bibliometric network, generated via the method described in the previous chapter. A full list of network members is contained in Appendix D. My approach to sampling from the 250 potential interviewees is described below.

5.1.1. Recruitment Priorities: A Wide Net

The sampling strategy was purposive, jointly informed by the following five recruitment priorities:

- Represent all clusters
- Represent a range of disciplines
- Represent a range of geographic locations
- Represent a range of career stages
- Represent a mix of high-profile and of lesser-known researchers

Alternative strategies may have been more targeted, focused intensively on a small number of clusters and/or disciplines. This will be an analytically productive design for future studies, however I felt there was not sufficient theoretical or empirical basis for ignoring any region of the network, and so I favoured surveying the network more broadly. This allowed me to remain open to all kinds of variation, and to lay a general empirical foundation for future, potentially more-narrow projects. Additionally, due to the lack of international comparative studies discussed in Chapter 2, it is an open question whether and how the influence of disciplinary paradigms varies across geographic or institutional contexts. I viewed being able to demonstrate that findings do (or don't) cut across a range of institutions and countries as beneficial. Finally, it was not known in advance which debates and questions were important to members of the network, and what kinds of professional and scientific challenges they experience. As existing research-on-research within HIDR has focused heavily on the UK and Europe, I felt it inappropriate to assume these findings extend to other contexts, other disciplines, institutions and career stages. Sampling widely across the network allowed me to potentially identify salient disciplinary dynamics which cut across diverse contexts, as well as to identify those which seem context-specific.

Prioritising inclusion of lesser-known researchers

As discussed in Chapter 2I did not want to limit my study to unusually large or expensive projects, or to prominent members of HIDR.

Commencing the project it was my sense that researchers appear to form opinions about 'the field' based on their own local networks, plus high-profile figures from other disciplines, and countries. An interview corpus dominated by the perspectives of well-known, highly-cited researchers (who have a platform to express their views, should they wish to) seemed like a missed opportunity to incorporate diversity. In addition, the difficulty of obtaining high quality individual-level data for scientometric work has tended to result in the summary of research domains via ‘league tables’ which rank only the most highly-cited authors. This can skew perception of a research area, because researchers at late career stages whose work is highly cited may have different experiences and hold different opinions about the state of their field. I felt that limiting data collection to ‘big names’ would waste an opportunity to gain insight into the views and perspectives of ‘up and coming’ researchers, or researchers whose work intersects between HIDR and other areas. I therefore prioritised recruitment of these researchers, by first inviting authors who are

I) located at the perimeter of their cluster

and/or

II) have published (relatively) fewer papers

Despite this effort, it is important to note that the bibliometric analysis already excluded researchers on the periphery of HIDR, and those emerging researchers who have published very few (1-4) relevant articles.

5.1.2 Restrictions on Sampling

Recruitment priorities and aims were balanced against the following restricting factors:

- An anticipated sample size of 30 interviewees
- Limited resources for travel to conduct interviews in-person
- Evolution of the bibliometric network (and therefore a changing pool of potential interviewees)

The sample size of 30 was selected as being reasonable in the time available. Mapping this into the network, keeping the five recruitment priorities in mind, I set recruitment targets for each cluster and identified a set of researchers to invite. The sample was ultimately expanded to 45 in order to increase the number of included researchers from the US, where responses to my invitations were slow, and few (see 5.1.3, below).

Overseas Travel

I travelled to conduct research interviews on three occasions, including five interviews conducted during a trip home to Australia. Feeling it was important to expand the sample beyond English-speaking Anglo-Saxon countries, I applied for funding from Edinburgh University's Principal's Go Abroad Fund to travel to the Netherlands and Spain, where I conducted nine interviews. These countries were chosen because they contained researchers in multiple network clusters and disciplines, and also because travel to these two countries was feasible, given the size of the grant. A fourth overseas trip to the US would have been ideal, however as network members in the US are spread across a very large geographic area there was not an obvious single location to visit, and resources did not permit a tour of multiple locations, and so a trip to the USA was not feasible.

An evolving network

To complete data collection within the timescale of a PhD, recruitment for interviews could not be delayed until after the bibliometric network was finalised. Recruitment therefore began based on an early iteration of the network completed in late 2017. For this reason, nine invited researchers do not appear among the 250 authors listed in Appendix D. Interviews were conducted with two of these nine (see Cluster=N/A in Table 4, below). One interview was with an economist, who generally expressed positions quite similar to other economists interviewed. The other interviewee was a statistician, who no longer works in an academic setting. Their data was not included in analysis.

5.1.3 Contacting Researchers

In total, 112 researchers were contacted via a blend of email and hard-copy invitations (explained in more detail below), with the first invitation sent in April 2018 and the last in May 2019. Invitations and acceptance rates are presented in Table 4, below:

Cluster	Invitations	Interviews	Response Rate
1	18	7	39%
2	16	9	56%
3	27	7	26%
4	18	11	61%
5	4	0	0%
6	5	3	60%
7	3	3	100%
8	12	3	25%
(N/A)	9	2	-
Total	112	45	40%

Table 4, Invitations and Interviews by Cluster

5.1.3.1 Interviewee Response to Recruitment

Responses to invitations varied by network cluster. Figure 2 depicts the distribution of interviewees across the HIDR network.

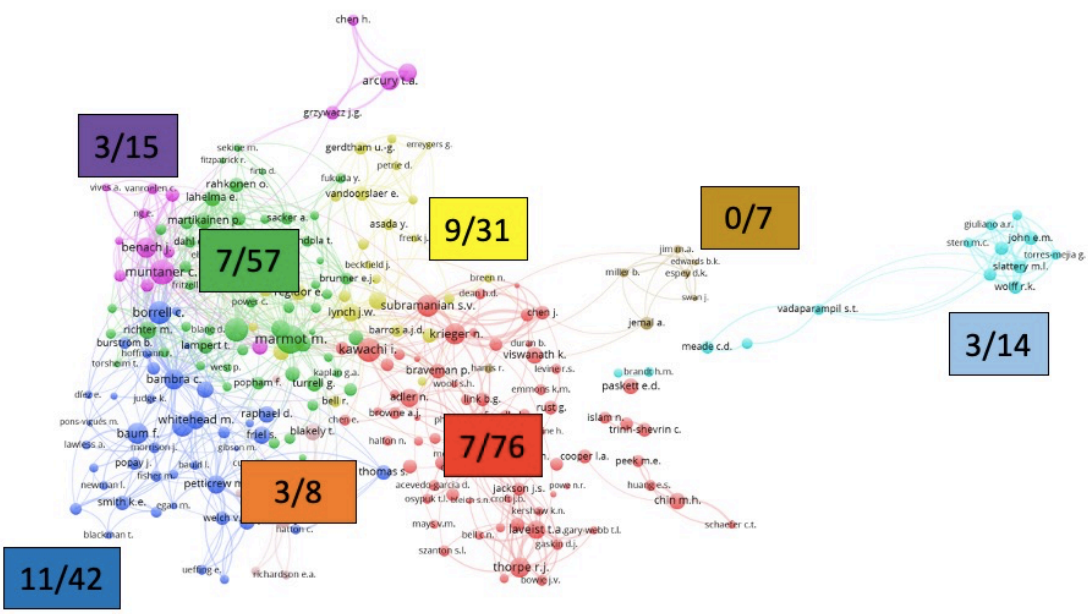

Figure 9: Distribution of interviewees across the bibliometric network

Low rate of response from North American Researchers

Response rates among North American researchers (especially Clusters 3, 5 and 8) were low. After very few researchers responded to my initial email invitation and follow-up email, hard-copy letters were sent and followed up via email, which proved a more effective approach.

The last interview I conducted was with an American epidemiologist, who commented on the length of time I had asked for:

> I think asking for an hour is really off-putting. I would suggest you ask for "a short time", 20 minutes or something. Everyone is so busy, to someone they dont know, it's like asking for $1000.
>
> Health Equity Researcher (Nursing PhD)

Attempting to schedule interviews with North American researchers (more likely than researchers from the UK or Europe to be clinicians and/or working in clinical settings) I did get a sense that the pace of academic life is rapid, and that asking for 1 hour really was like asking for $1000. This may explain the difficulty I faced in recruiting researchers from the US, and points to the manner in which my study design selects against researchers with packed schedules, who may have a particular kind of experience missing from my interview data.

Cluster 5, comprised chiefly of public servants and career statisticians at the National Institutes of Health and the National Cancer Institute, proved especially challenging. Some members of Cluster 5 have retired and could not be contacted. All members of Cluster 5 whose email addresses are public were repeatedly contacted, however none responded.

Interviewee Characteristics

In total, 45 interviews were conducted across eight countries, seven clusters and 16 doctoral disciplines. Interviewee location and mode of conduct are presented in Table 5, and interviewee's PhD disciplines are listed in Table 6.

Country	In Person	Phone	Skype	Total
UK	8	1	5	14
USA		2	11	13
NED	3		2	5
AUS	2		2	4
CAN			3	3
SPAIN	3			3
NOR			2	2
NZ			1	1
Total	15	3	28	45

Table 5, Interviewee Location and Mode of Conduct

PhD Discipline	N
Epidemiology	9
Economics	5
Public Health	4
Biostatistics	3
Geography	3
Medicine	3
Sociology	3
Anthropology	2
Demography	2
Nursing	2
Psychology	2
Public/Social Policy	3
Health Policy	1
Health Promotion	1
Health Services Research	1
Political Science	1
Total	45

Table 6, Interviewee PhD disciplines

While Table 3 presents a general sense of interviewees' disciplinary trainings, interviewees' *disciplinary identity* did not always align with their PhD discipline. Several interviewees holding PhDs in epidemiology or public health identified as members of their undergraduate or Masters discipline (Such as Political Science, Sociology, Geography, History or Medicine), and vice versa. To provide as much detail without identifying interviewees, in the thesis I refer to interviewees via the field contained in their title at the time of interview (e.g., 'Research Fellow, Biostatistics' becomes "Biostatistics"), plus the discipline of their PhD, and (where relevant) the bibliometric cluster. PhD discipline was categorised on the basis of either:

I) Description of interviewees' PhD in their CV or most-recent online profile

II) Description of PhD during the interview.

Where interviewees did not discuss their PhD in the interview, the former data source was relied upon.

Four social scientists interviewed are clearly identifiable via the combination of their current title and PhD discipline. After contacting two of these interviewees[7] requesting input into the way they are identified, I elected to refer to this group as holding “Social Science” PhDs, rather than naming the specific social science.

5.2 Interview duration

The majority of interviews lasted between 50 and 65 minutes. Four interviewees had just 30 minutes to spare, thankfully I knew this in advance and was able to prepare. Three interviewees had a lot to say and discussion lasted in excess of 90 minutes.

5.3 Interview content

Each interviewee received a consent form (Appendix E) to read and sign prior to the date of interview. The main points of the consent form were repeated orally at the beginning of the interview, and interviewees’ right to terminate the interview was reinforced. Consent was separately obtained from all interviewees for me to record the conversation (See section 5.2.7 for a discussion of how interviewee identification was presented on the consent form). Interviews had a semi-structured approach where a general set of topics were explored using a thematic interview schedule (described in detail in Appendix A) covering the following:

1. Overview of interviewee’s academic background, disciplinary identity, perceived strengths and weaknesses of own-discipline, management of disciplinary identity.

2. Mental models of processes which cause health inequalities/ disparities

[7] Two of the four had requested to view any extracts included in the thesis for identifying information.

3. Perceived key debates/methodological issues within HIDR
4. Experiences collaborating with researchers from other disciplines
5. Reactions to the literature: The hallmarks of ‘good’ and ‘frustrating’ research.
6. Discussion of statistical issues and experiences with statistical collaborators.
7. Looking to the future; ideas, methods or individuals perceived as likely to be important within HIDR in the coming years.

I developed a short activity to provide an additional way for participants to talk about their own discipline, and other disciplines, and to provide a change of atmosphere toward the end of the interview after interviewees had been talking for some time. Researchers were asked to identify a determinant of health which they had studied or were studying, and invited to imagine they had funding to allocate among five potential research projects. Interviewees were asked to rank the studies below by moving printed cards around the table. For Skype interviews, researchers were emailed a Word document containing these studies and asked to rank them.

- A biomedical study seeking to understand the physiological pathways impacted by X
- A geographic study focused on the spatial distribution of X
- A randomised implementation of an awareness campaign about X
- A qualitative study exploring the lived experience of individuals exposed to X
- A secondary analysis of administrative data, investigating the link between education, income and X

Several researchers objected to one or all of the above studies, wanted to add an extra study, or to make changes to the prescribed designs. This proved a valuable way to begin a discussion about research methods and forms of evidence. Some interviewees reflected that they had been confronted with similar choices during experience on grant funding panels, and then went on to discuss the disciplinary dynamics of such panels. Others commented that the activity was a useful thought-experiment, which they enjoyed:

> That was interesting, that was a good thought-experience, to go through that.
>
> Health Geographer (Geography PhD)

The inclusion of a qualitative study among the five options was particularly important, as this prompted quantitative researchers to reflect on qualitative research methods without me having to ask “Do you value qualitative research?” directly. A qualitative study concerned specifically with lived experience is not the only kind of qualitative research of relevance to HIDR, and I could have included other kinds of qualitative research, however this was sufficient to begin a useful conversation.

Where time was limited this activity was not conducted, or I asked researchers about their “dream project” which they would conduct if funding was unlimited.

5.4 Interview Conduct

5.4.1 Pilot Phase

As I did not have any experience with or exposure to qualitative research prior to this PhD, I conducted a set of pilot interviews to improve my familiarity with the method and iron out any practical issues (e.g., with recording equipment). Four pilot interviews were conducted in-person with academics from the University of Edinburgh in different disciplines, helping to build my confidence with the methodology and refine the interview schedule. Prior experience in market research helped me steer interviewees towards areas of interest and away from irrelevant topics. Feedback from pilot interviewees was used to improve my awkward handling of interview logistics such as ‘small-talk’ before and after the interview, and delivery of a pithy summary of the project to help orient interviewees. One pilot interviewee (themselves an experienced qualitative researcher) generously met with me to discuss areas for improvement. Pilot data are not included in the results, as pilot interviewees were not HIDR researchers (or members of the bibliometric network).

5.4.2 Interview Mode: Skype and In-Person Conduct

The majority of interviews were conducted via Skype (see Table 2). This is perhaps unfortunate, as online meetings do feel different to in-person encounters. I was not able to travel to the US to conduct in-person interviews, a clear limitation of my data. Verbal and non-verbal cues are accessible via Skype (Sullivan, 2012), but meeting in-person with interviewees in Spain, the Netherlands, the UK and Australia, I was able to get a sense of their professional environments and a short glimpse into their academic institutions, helping to understand the 'small lifeworlds' (Knorr-Cetina, 1999) within which research is conducted. Building rapport also felt easier in person. Reviewing my field notes, in two cases I specifically mentioned that a Skype encounter felt superficial. Having said this, academics are a social group with significant experience conducting meetings online. Most interviewees had no trouble with the technology, and seemed very comfortable with the format. Many interviewees spoke with me from their homes, with pets, children and spouses occasionally wandering into view. This is perhaps a different kind of small lifeworld, equally important in an academic career, which I could not have accessed via another method. The distance between interviewer and interviewee during an online discussion may also increase candidness compared with in-person interviews (Bargh et al., 2002, Ellison et al., 2006). I certainly felt a sense of comfort and confidence being able to conduct interviews from my own desk, or my own home.

Additionally, I was able to freely write during these interviews without distracting the interviewee, which meant I could carefully formulate follow-up questions and make a note to clarify an interesting point. In person, some interviewees seemed put-off by me scribbling notes during the discussion, and I found myself occasionally forgetting which points I intended to probe if I did not keep notes (especially late in the day). Another advantage of the online format was that interviews could occur at any time of day or night, and if an interview needed to be rescheduled with little notice (as happened several times) I had not wasted time or resources travelling to the interviewee's location.

Due to technical issues, or interviewees not having access to videoconference technology, three interviews were conducted by phone and these were quite challenging. Without visual cues it is difficult to cut-off or redirect an interviewee without appearing rude, and so I was not able to make the best use of the time available. Nevertheless, these phone interviews yielded interesting insights and all three have extracts included in the thesis.

At the time of submission, COVID-19 has normalised videoconference as a social encounter. My experience conducting a large number of interviews digitally in 2018/2019 feels quite valuable now, and I have enjoyed sharing my experience with other PhD students as they prepare to collect their data digitally.

5.5 Evolution of the interview schedule

The interview schedule evolved as interviews progressed, three major changes are discussed below.

5.5.1 Disciplinary Strengths and Weaknesses

One interviewee spontaneously offered some reflection on the strengths and weaknesses of their home discipline early in the interview, which struck me as a useful window into my areas of interest. For this reason, I began asking some variation on the following in the introductory section of the interview:

> "If there was one concept from [your discipline] which you could snap your fingers and have understood more widely within population health, what would it be?"

This proved a good way to get interviewees talking about what they perceived as the strength of their discipline (which all interviewees could do with clarity and confidence) and also their views about where that discipline sits within the wider landscape of population health, within HIDR, key contrasts with other disciplines, and the extent to which their discipline is misunderstood.

5.5.2 Debates

Like other students conducting interviews for the first time, I began with certain ideas about my topic which proved to be laughably simplistic. I had originally identified one debate which I planned to ask researchers to comment upon, but it became apparent that researchers from different disciplines and countries not only had different positions within this debate, but also had different perspectives on *whether the debate was happening*. As the latter was just as interesting as the former, this theme became more open-ended as data collection progressed, and I simply asked whether interviewees were following any particular debates.

5.5.3 Statistics

Having trained as a statistician I was extremely interested in interviewees' views on statistical issues, and disciplinary variation in those views. As the sociology of statistical practice does not currently exist as a mature specialty (see Chapter 9), support in the literature to guide questioning on this theme was scarce. Questions were developed more out of my own professional experience than by previous empirical work. Perhaps for this reason this theme evolved throughout data collection. It became immediately apparent that even experienced researchers with advanced statistical training felt unsure when discussing statistical issues, especially "with a statistician". Rather than asking about specific methods or issues, I found that simply saying "now I would like to discuss statistical issues" was sufficient to begin an interesting discussion in most cases. For researchers without quantitative research experience, I began by asking what they looked for in a statistical collaborator, and what led them to trust a statistician. Data generated by this line of questioning was so interesting that I began asking every interviewee this question.

5.6 Challenges

5.6.1 Politics

As this thesis is focused on research contexts, I was initially concerned when some interviewees discussed engagement with policy makers, as this ground has been covered by other researchers, at least in the UK (e.g. Garthwaite et al., 2016). I initially considered the realm of policy to be separate from research, outside the scope of my project. However, as data collection progressed (and my own understanding of the relationship between research and policy became more nuanced) I saw that researchers' orientation toward or away from political engagement represented a point of variation across disciplines, reflecting underlying commitments about what it means to 'do science'. While I did not ask interviewees directly about engagement with policy, I did not discourage interviewees from sharing views and experiences on this subject.

5.6.2 Mishaps and avoidance

Thankfully, no catastrophes occurred during my interviews. I was offered employment on the strength of my statistical background the end of two interviews, which was surprising, but no overtly inappropriate or awkward exchanges took place. No interviews became heated, and, reviewing the transcripts and my field notes I cannot detect any instances of obvious evasion or avoidance. Several times it was obvious I was taking the discussion in a direction my interviewee was reluctant to go, for example:

> There may be reasons why those studies weren't well designed. Our public health colleagues more than the epidemiological colleagues, in my view, have a proclivity for rejecting scrutiny. [Pause] That was pretty strong, wasn't it?
>
> TC: It's a more interesting interview if you tell me what you really think. So I'm clear, that distinction between 'public health' and 'epidemiological researchers', what kind of training do those 'public health' researchers have?
>
> Arrrrggh. [Pause]. We are starting to stray into difficult territory here.
>
> [Long pause]. They may not have the same quantitative skills or the same

> deductive thinking about causal inference, that concerns me.
>
> Epidemiologist & Medical Doctor (Medicine PhD)

In cases, like the above, interviewees did eventually share their opinions (at no time did an interviewee refuse to answer a question). Generally, interviewees were enthusiastic about the project and seemed to enjoy the discussion:

> I think it's going to be a really interesting study. I enjoyed the conversation.
>
> Public Health Researcher (Demography PhD)
>
> This topic interests me a lot, I would love to read your dissertation.
>
> Public Health Researcher & Medical Doctor (Health Policy PhD)
>
> This was very interesting. Difficult questions sometimes, but still good to think about. Really interesting. I am looking forward to what comes out.
>
> Epidemiologist B (Epidemiology PhD)
>
> This seems like a really interesting study. I'm glad you're doing this.
>
> Social Epidemiologist A (Epidemiology PhD)

My lack of familiarity with the field of geography led to some unfortunate exchanges where I repeatedly confused geography with geology, however these interviewees were graciously forgiving.

One interviewee who had trained and worked solely in clinical epidemiology struggled with almost all of my questions and did not appear to have reflected on the status of epidemiology within public health, or been exposed to critical views of epidemiology. This was a very interesting discussion but in hindsight I could have better minimised the sense of awkwardness.

Some minor mishaps included neglecting to take a pen to one interview and forgetting several follow-up questions as a result, travelling to the wrong city for one interview (which was subsequently rescheduled), a mix up with a room booking resulting in an interrupted Skype interview, and my digital recorder's hard-drive failing during one interview, which was remedied by a hasty switch to the recorder on my phone. These last two mishaps were rattling, and distracting, but in the spectrum of possible issues I could have faced, my interviews went extremely well, especially given my lack of experience with this methodological approach.

5.6.3 Embracing Qualitative Research

I struggled initially to envisage how interview data could be harnessed analytically. My statistical training instilled in me a persistent, nagging concern about collecting the “right” data to answer my research questions. While I had read, and was assured, that it was not essential to ask every interviewee exactly the same questions, due to my inexperience I didn’t understand how interviews could produce analysable data in the absence of such structure. For this reason a deep panic set in toward the end of my pilot interviews (which in hindsight were really a sort of oral survey) that nothing interesting would come from my interviews. Gradually, as interviewees began to contradict each other and themselves in interesting ways, I realised that I was making headway relevant to my aims. I began to understand that it cannot be known in advance what the “right” kind of interview data is, and that one strength of interviews is that they can reveal the complexity of social experience, and challenge simplistic assumptions held by the researcher.

Later, as I entered the analysis phase, I experienced the challenges of moving beyond simple description in qualitative analysis. I struggled with the balance between interview extracts and my own analysis, and also with the challenge of finding a coherent analytical path through interviewees’ diverse perspectives. But ultimately, connecting theory, my research interest, and the experiences of my interviewees has been a deeply rewarding experience.

5.7 Preparing for Analysis

5.7.1 Preparing & Anonymising Interview Transcripts

I transcribed all 43 included transcripts. References to specific individuals, degree programs, institutions and cities were replaced with [Blank].

Additionally, all disciplines are spelled via UK conventions (e.g., ‘health behaviour’ not ‘health behavior’). In the consent form (Appendix E), interviewees could express a preference for being associated by name with their comments. However, as less than a quarter of interviewees expressed this preference, and this inconsistency felt quite distracting as the chapters took shape, and for this reason, I decided not to name any interviewee in the thesis.

The consent form also provided interviewees with the option to review the transcript of their interview, and comment on issues relating to anonymity and accuracy. Nineteen interviewees requested their transcripts and these were sent in May 2019, requesting a response within two weeks (and noting that I would interpret a lack of response as an indication that the interviewee was happy with the transcript). No interviewee requested substantive changes their transcript, although two interviewees were concerned about being potentially identified in publications, and wished me to check specific extracts prior to submitting, which I have done.

Whilst I made every effort to protect the identity of my interviewees I am aware of a small number disclosing their participation to other interviewees. For example, on one overseas trip I interviewed multiple members of the same institution, and overheard them discussing the project on my way out. Additionally, I discovered that two of my interviewees were married to each other, however I had not realised this connection at the time of interview.

Other than the nineteen interviewees who requested their transcripts, only I have seen the interview transcripts. My supervisors assisted me in making decisions about how to maintain anonymity whilst providing sufficient context for the reader, and on a few occasions helped to select the best extract relevant to an analytical point (from a small number of candidates), but otherwise I did not discuss the content of interviews with my supervisors, or provide them with any extracts outside the analysis described in this thesis.

5.7.2 Data analysis

Early in the PhD, analysis of qualitative data seemed mysterious. I completed mandatory data analysis training, and was overwhelmed by the analytical options. I went down several methodological rabbit-holes, and grew enthusiastic about some specialised approaches (sociolinguistics, semiotics, discourse analysis), however I ultimately abandoned these in favour of thematic analysis. Thematic analysis seemed to be the approach taken to produce work I greatly admired, including work which had addressed questions similar to my own (Smith, 2007; Knorr-Cetina, 1999; Rudwick, 1985; Lamont, 2009; Bloor, 2011). Braun and Clarke (2006) elaborate a 6-step guide to thematic analysis, which I followed. Whilst Braun and Clarke stress the 'fuzziness' and non-linearity of the method in their article, it is only now, at the end of the project, that I have a sense of what this means in practice.

Even with this training and guidance I struggled to locate (and to imagine) the intermediate products of thematic analysis, and the diagnostic tools which could assure that my conclusions appropriately reflected the data. Weekly tutorials in my data analysis course frequently involved a session where analytical or methodological ideas about a topic were written onto post-it notes and organised into groups on a white board[8]. I found this way of working extremely helpful, and liked that early- and late-breaking ideas were clearly visible alongside one another. I also liked physically engaging with ideas in this way, without a computer screen

I used the same approach in my thematic analysis. To build results chapters, codes were reviewed and all relevant extracts summarised on post-it notes. These were then shuffled around into groups, and summarised via excel spreadsheets, which informed the outline of draft chapters. An example of one spreadsheet (informing the discussion of theory in Chapter 8) is included as Appendix G.

When drafting results chapters I was mindful of the below guidance from

[8] The point was not to demonstrate this methodology, but to assist in arriving at a position or set of positions which the tutorial group would then investigate, in small groups.

Fontana & Frey:

> Many studies using unstructured interviews are not reflexive enough about the interpreting process; common platitudes proclaim that the data speak for themselves, that the researcher is neutral, unbiased, and "invisible." […] there are no contradictory data and no mention of what data were excluded and/or why. […] The main concern seems to be the proper, if unreflexive, filing, analyzing, and reporting of events. But anyone who has engaged in fieldwork knows better (Fontana & Frey, 2000: p.87)

I therefore approached my interview data with an awareness that it was generated via a process I designed, and that I determine, at least via inclusion or omission, what data are permitted to 'say' in this thesis. To try to report as transparently as possible, I began each analysis by exhaustively reviewing relevant codes, summarising each potentially relevant extract onto a post-it note. This ensured that all available data was (literally) on the table at the beginning of analysis. I actively searched for interview data complicating or cutting against my main findings, and, after finalising the thesis draft, confirmed that all interviewees do have data included in the thesis.

5.7.3 Coding

Transcripts were coded using the open-source R package RQDA (Huang, 2008; v0.3.1). RQDA is clunky, however has the advantage of being free, and situates coded extracts, memos and journal entries within an SQL database which can be subjected to complex queries, exported for backup, and analysed via other tools. During transcription I kept notes of broad themes which seemed important, and was able to begin coding with a list of initial codes. As is inevitable, I returned to early interviews having added codes part-way through analysis. In total I coded all transcripts at least twice and used 169 codes (listed in Appendix F).

5.8 Strengths & Limitations

I have discussed strengths and limitations throughout these methods chapters, but briefly summarise them here. First, the inclusion of interviewees from a variety of countries and continents is a major strength, allowing me to identify trends and contrasts cutting across geographical contexts. Secondly, sampling interviewees from a bibliometric analysis (rather than convenience or snowball sampling) is likely to provide a better view of the field. However, bibliometric data has major limitations, discussed in Chapter 4. In addition, this geographical breadth has resulted in a survey of the field which may be viewed as shallow. However, the bibliometric analysis provides a sampling frame for future, more in-depth projects.

5.9 My Disciplinary Training

A thesis focused on the impact of disciplinary training on knowledge construction demands some reflection on the author's own training. My background has clearly shaped the focus and content of this thesis. From the beginning of my undergraduate training I have been simultaneously studying health from multiple disciplinary perspectives, but nobody can study health from *all* perspectives, and so there is a slant toward understanding the disciplines with which I have first-hand experience.

Epidemiology and economics feature prominently in the thesis, perhaps where other disciplines may have been the focus had I trained in other fields. I had originally intended to focus *exclusively* on epidemiology and economics (to focus on the 'schism' identified in Chapter 1) but when reviewing results of the bibliometric analysis it became clear that this was an unacceptably narrow account of the disciplines contributing to HIDR.

I have more experience in epidemiology and biostatistics than the other disciplines covered in the thesis, and more than once my supervisors noted an apparent lack of balance in some sections, where I seemed to be taking the epidemiological position as the 'default' which did not require explanation or careful analysis. This is something which many of my interviewees trained in epidemiology did in interviews. In this way, my own gradual transition from a disease-focussed statistician to social scientist lead me to encounter many of the issues discussed by interviewees who attempt to blend sociological and epidemiological accounts of health in their research.

My statistics background has influenced the thesis in several obvious ways, including the focus on method throughout the results, and the presence of a chapter dedicated to statistical methods. My experience in biostatistics consulting initially suggested that researchers from different disciplines approach the same method in different ways. Perhaps tellingly, it was in Chapter 9 (focused on statistics, the discipline with which I identify most closely), that I struggled most to maintain a balanced analytical stance, and to sufficiently explain my analytical choices. In that chapter, interviewees are divided into four groups on the basis of their use of statistics, and in initial drafts I neglected to provide any explanation of this process, as the validity of my chosen groupings seemed obvious (to me). Additionally, my training in statistics clearly prepared me to recognise the diverse balances disciplines strike between positive and negative knowledges (Knorr-Cetina, 1999) discussed in Chapter 7.

My training in economics does not mean I bring an un-critical eye to that discipline, but I am certainly more sympathetic to economic approaches than most interviewees seemed to be. According to my own analysis (see Chapter 8) this is likely because I have a detailed understanding of the kinds of questions economic approaches were developed to answer, and view those approaches as appropriate and useful for answering those questions, even if they are not questions central to HIDR. My experiences observing the way economics is perceived by researchers from other backgrounds in population health drew me initially to consider the Kuhnian lens, as that lens seemed to make sense of my own experiences.

Generally, my training and experience working diverse in research contexts served as an important resource in the thesis, including in motivating the research question. There is no existing literature to read which describes the near-total epistemological disconnect I experienced studying health economics and biomedical science as an undergraduate, and, despite the challenges of striving for balanced analysis (despite my unbalanced background), were it not for that training and those experiences, the thesis would not exist.

Chapter 6: The Tribe and The Territory - The Health Inequalities and Disparities Research Network

> ”There are disadvantages in relying solely on the description of an area delineated in terms of publications and citations ... it is all too easy ... to think of its boundaries as objectively independent of participants”
> Latour & Woolgar, Laboratory Life (1986:p.114)

In this chapter, I address the first aim of the thesis via presentation of the author-level bibliometric analysis results, and demographic data capture. This analysis provides a detailed picture of the field’s 250 most-connected authors, their arrangement in citation-space and their disciplinary backgrounds. Results demonstrate that an extremely diverse range of disciplinary trainings are present within HIDR, and that due to geographic, institutional and disciplinary ‘siloing’ , disciplinary diversity is not equally distributed across the field. I also draw upon interview data to place the bibliometric network within a historical context, which helps to inform understanding of the network’s structure.[9]

6.1 The Network

Figure 10 is the result of the network-generation process described in Chapter 4. In total, 29,212 papers containing relevant keywords were extracted from Scopus, from over 8,500 authors. Citation flows between pairs of authors were analysed and the 250 most-connected authors were identified, and arranged in space such that authors with strong citation links are located close together, producing Figure 10, below.

[9] Appendix H is a peer-reviewed manuscript published in *Social Science and Medicine* drawn from this chapter. This paper is co-authored by one of my supervisors, but, as is reflected in the contribution statement, her contribution was limited to the introduction and concluding discussions, which are not reproduced in this thesis.

Figure 10, The 250 most-connected researchers within global HIDR.

Nodes represent authors who have published at least 5 papers with relevant keywords. The size of the node/circle represents the number of papers each author has published. Width of lines indicates the number of citations between authors. The colour of the nodes represent different clusters (numbered 1-8) of authors detected by the clustering algorithm described in Chapter 4.

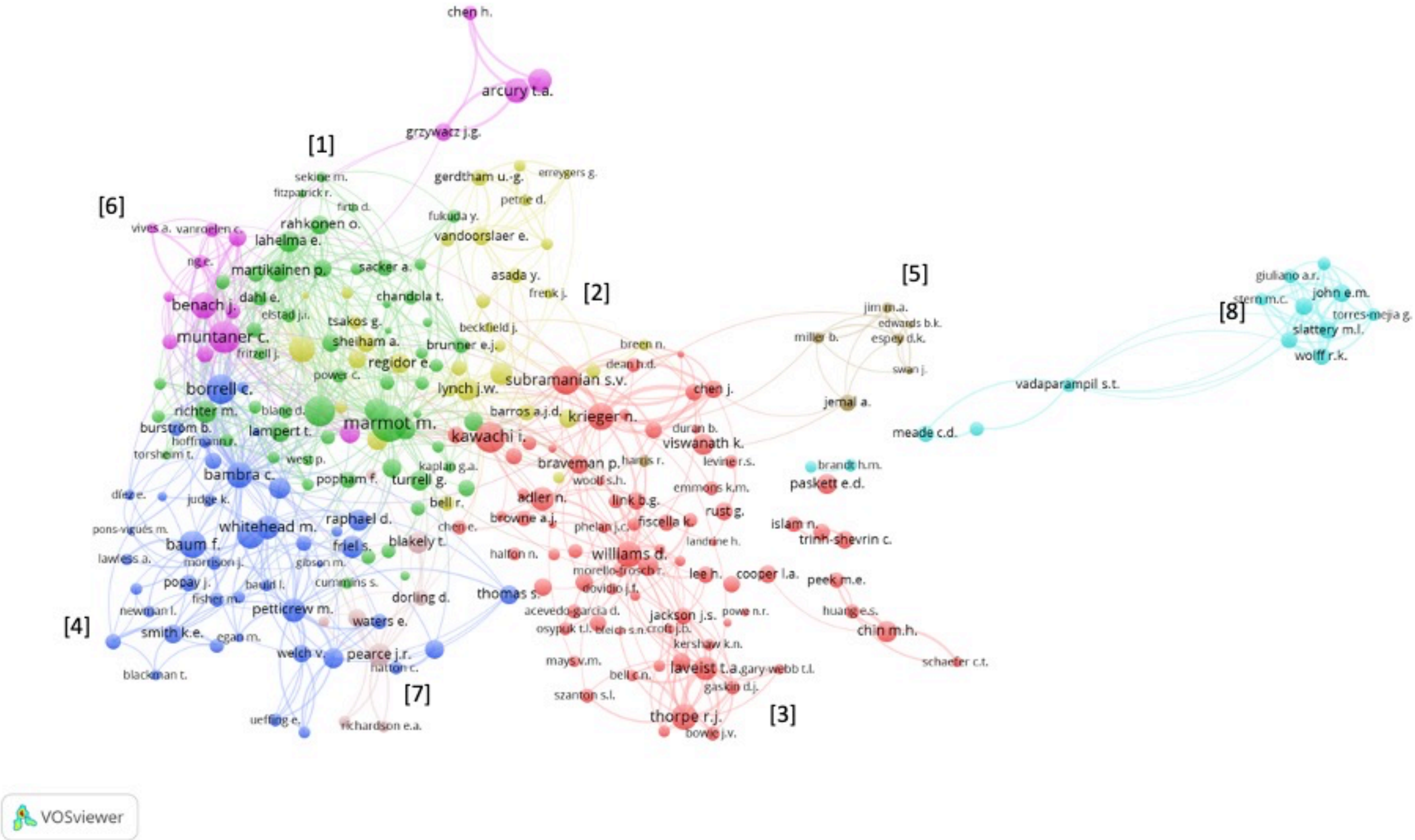

Figure 10 depicts the 250 most-connected authors publishing research in English about health inequalities or disparities between 1976 and 2016 (hereafter, "researchers" or "authors"). Despite spanning 40 years, 25,165 of the 29,212 papers (86%) used to create Figure 10 were published between 2008 and 2016, reflecting the exponential increase in publications, noted by existing reviews of the field (Cash-Gibson et al., 2018). To investigate the extent to which disciplinary training is evenly distributed, the network was divided algorithmically into eight citation clusters, regions of the network where a high proportion of citations are local (see Section 4.3.7).

6.1.1 Network Morphology

The left and right halves of the network have different spatial arrangements and citation structures; the left being made up of five densely connected, partly-overlapping[10] clusters (labelled 1, 2, 6, 7 and 4 in Figure 10). The right hand side comprises three non-overlapping clusters with relatively sparse interconnectivity (labelled 5, 8 and 3). Generally, though with multiple exceptions, researchers from the US appear on the right hand side of Figure 10 and researchers from the UK, Europe, Australia, Canada and Europe on the left. Citation flows appear to be determined by a mixture of geographic, methodological, institutional and disciplinary factors. For example, a group of social scientists from the UK appears in the bottom left-hand region, and a group of economists from various countries are located at the top of Figure 10. Cluster 8 is comprised almost exclusively of co-investigators from a single project, and Cluster 5 from researchers affiliated with the National Institutes of Health and National Cancer Institutes. Demographic information about network members may help explain the form of Figure 10, presented in the next section.

[10] Figure 10 is a 2-dimensional presentation of a three-dimensional object. Whilst it appears that some members of cluster 4 (yellow) are embedded among researchers in Cluster 2 (green), it is more likely that these researchers jump out of the page toward the reader, and that Clusters 4 and 5 wrap around the central cluster.

6.2 Network Geography, and Academic Degrees

Country	Count	%
US	108	43%
UK	59	24%
Canada	19	8%
Australia	12	5%
Netherlands	8	3%
Germany	7	3%
Spain	7	3%
Sweden	5	2%
Brazil	3	1%
Finland	3	1%
Norway	3	1%
Belgium	2	1%
Chile	2	1%
Japan	2	1%
South Korea	2	1%
New Zealand	2	1%
Switzerland	2	1%
Other	4	2%
Grand Total	250	100%

Table 7: Network Members' Geographical Location

The geographic location of the network's membership is summarised in Table 7. Two thirds of researchers are based in the US or UK, and an additional 13% in Canada or Australia.

The remainder of the network comprises of researchers from Europe, Latin America and South East Asia. The prominence of researchers from the US and UK is expected, as the most established HIDR research and training programmes are in these countries. Cash-Gibson and colleagues (2018) reported that the Anglo-Saxon nations (US, UK, Canada, Australia) produced around 70% of HIDR's scientific output between 1966 and 2015, and the most-connected 250 authors within HIDR appear similarly concentrated. No low- or low-middle-income countries are represented in the network (with the exception of Brazil, an upper-middle income country, only high-income countries are represented). No researchers are located in

China, Russia, or the African continent. Since bibliometric methodology favours established authors (who have been publishing for longer) these are more likely to have accumulated the citations required to appear in the network. It may be that recent contributors from other geographic regions not captured in Figure 10. However, some authors within the network published their first paper in HIDR as recently as 2013, ruling this out as a complete explanation. In analysis of geographic and regional trends within HIDR, Cash-Gibson and colleagues report that geographical disparities in HIDR production are extant, and widening (Cash-Gibson et al., 2018).

Table 8 includes a breakdown of first degree and highest degree (MD, Juris Doctor, DPhil, etc.) by subject category. A wide range of natural, medical and social sciences are represented in network members' first degrees, as is geography, mathematics, statistics, literature, and engineering.

Unsurprisingly, the most common category for highest degree is 'Public, Environmental & occupational health' (which includes epidemiology, health promotion, health behaviour and health education), followed by sociology, medicine, psychology, economics/health economics, and political science. Almost one in five network members have a medical qualification.

This study is the first detailed picture of the disciplinary backgrounds underpinning a population health research sub-field. From Anthropology to Zoology, Theology and Leather Technology, almost every subject area is represented in the background of researchers in Figure 10. The presence of a variety of trainings suggests the

Subject Category	First Degree	PhD (Or Highest Degree)
Public, environmental & occupational health	6	74
Sociology	27	32
Medicine, general & internal	44	22
Psychology	26	13
Economics	11	12
Political Science	8	11
Geography	12	8
Social sciences, biomedical	3	8
Statistics & probability	3	8
Psychology, clinical		7
Nursing	8	5
Demography		4
Health policy & services		4
Biochemistry & molecular biology	2	3
Health care sciences & services	8	3
Medicine, research & experimental	2	3
Social sciences, interdisciplinary	7	3
Anthropology	2	2
Behavioural sciences		2
Dentistry, oral surgery & medicine	3	2
Ecology	1	2
History	7	2
Social work	2	2
Urban Studies		2
Biology	15	1
Business		1
Communication		1
Education & educational research		1
English/Literature	7	1
Family studies		1
Genetics & heredity	1	1
Information science & library science	1	1
Law		1
Nutrition & dietetics		1
Philosophy		1
Planning & development		1
Chemistry	6	
Design	1	
Engineering	2	
Management	1	
Mathematics	5	
Microbiology	2	
Multidisciplinary sciences	3	
Neurosciences	1	
Pharmacology & pharmacy	2	
Public administration	3	
Religious Studies	2	
Veterinary sciences	1	
Zoology	2	
Unknown	13	4
Total	250	250

Table 8 : First Degree and PhD/Highest Degree by Subject Category

Data obtained from CVs, online profiles, and (where necessary) from researchers directly via email presence of diverse ideas about health and health equity, and Table 8 suggests that the most common ideas may be grounded in epidemiological, sociological, medical, psychological, economic, and political science paradigms. In the next section I investigate the distribution of these trainings across Figure 10.

6.3 Network Clusters

The 8 clusters identified represent regions where a (relatively) high proportion of citations are local. Combining the demographic dataset with the citation network, it is possible to describe how certain author characteristics distribute across Figure 10, including the extent to which disciplinary diversity is uniform across the network. Table 9 contains key details for each cluster, including its size, proportion of researchers located in the US/UK, proportion of researchers with medical training, and a breakdown of cluster members' highest degree by subject category. The data in Table 9 allow each cluster to be examined in detail, however these descriptive features are general and do not necessarily reflect all cluster members.

Diversity

Table 9 includes two statistical measures of diversity (described Section 4.6): the Integration Index and Shannon Number Equivalent (SNE) Index The Integration Index accounts for the abundance, evenness and similarity[11] of disciplines within each cluster. The SNE index accounts only for the abundance of disciplines and the evenness of their representation. As a reminder, the SNE index can interpreted as the *number of equally-represented disciplines* required to achieve the diversity of each community (Jost, 2007). The entire network of 250 authors has a SNE of 14.15, diversity equivalent to approximately 14 equally-represented disciplines (250 / 14 ~18 examples of

[11] For example, using this measure, the combination of medicine and physics within a cluster counts as being 'more diverse' than the combination of more similar disciplines, such as medicine and biology.

Cluster	1	2	3	4	5	6	7	8	Total
(n)	57	31	76	42	7	15	8	14	250
Number Equivalent Shannon Index	9.68	4.53	9.68	13.07	3.6	8.33	4.01	3.42	14.15
Integration Index	0.60	0.58	0.60	0.72	0.45	0.61	0.61	0.46	0.68
% US	7%	19%	96%	0%	86%	26%	0%	93%	43%
% UK	40%	19%	1%	52%	0%	0%	88%	0%	24%
% Any Medical Degree	18%	32%	20%	12%	29%	40%	13%	7%	20%
Median year of first included publication	1999	2002	2004	2005	2005	2006	2008.5	2011.5	2004
Earliest first included publication	1985	1997	1993	1983	2002	1999	2001	2003	1983
Subject Category: PhD/Highest Degree					**(% of cluster)**				**(% network)**
Public, Environmental & Occupational Health	19%	45%	36%	19%	43%	20%	0%	64%	30.00%
Medicine, general & internal	4%	6%	14%	5%	29%	13%	13%		8.80%
Medicine, research & experimental					14%	13%			1.20%
Nursing			5%					7%	2.00%
Dentistry, oral surgery & medicine		3%	1%						0.80%
Nutrition & dietetics			1%						0.40%
Psychology	11%		5%	5%		7%			5.20%
Psychology, clinical			8%	2%					2.80%
Behavioral sciences			3%						0.80%
Education & educational research				2%					0.40%
Statistics & probability	7%		1%		14%	7%		7%	3.20%
Biochemistry & molecular biology	4%							7%	1.20%
Ecology	2%						13%		0.80%
Biology								7%	0.40%
Genetics & heredity								7%	0.40%

Cluster	1	2	3	4	5	6	7	8	Total
Health policy & services			3%			13%			1.60%
Health care sciences & services	2%			5%					1.20%
Planning & development				2%					0.40%
Family studies						7%			0.40%
Demography	4%	3%		2%					1.60%
Urban studies	2%			2%					0.80%
Geography	2%		1%	5%			50%		3.20%
Communication			1%						0.40%
Business			1%						0.40%
Economics	2%	29%	3%						4.80%
Social sciences, biomedical	4%		1%	7%			13%		2.80%
Social sciences, interdisciplinary	2%			2%		7%			1.20%
Social work	2%		1%						0.80%
Sociology	26%	6%	9%	17%			13%		12.40%
Political science	4%	3%	3%	14%					4.40%
Anthropology						13%			0.80%
Law			1%						0.40%
History		3%		2%					0.80%
Literature				2%					0.40%
Philosophy				2%					0.40%
Information science & library science				2%					0.40%
Unknown	7%								2.00%
Total	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100.00%**

each discipline) across the network.

With the exception of Cluster 4, no cluster has the Shannon diversity or Integration of the whole network, suggesting a degree of disciplinary sorting or concentration within clusters. The following section explores this possibility by examining each cluster in turn, in chronological order.

Cluster 1: Whitehall Investigators & Health Inequalities Pioneers (UK/Europe).

The network's oldest cluster (by median entry to the field) is comprised largely of researchers from the UK and Europe who began studying health inequalities during the 1980’s and 1990’s. The relatively high diversity scores reflect the wide-ranging backgrounds of these early inequality scholars, which included many social scientists. This cluster is especially notable for its high proportion of sociology PhDs, making up just over a quarter of the cluster, while psychology, the social sciences and political science are also represented. The upper left part this of cluster includes several sociologists from Scandinavia and Germany. Researchers toward the bottom of this cluster (overlapping Cluster 7) share a focus on place and health.

Cluster 2: Economic approaches & measurement of inequity at scale (Netherlands, UK, US).

Cluster 2 is comprised of two distinct regions, each with disciplinary features. Work in this cluster emerged in the early 2000’s and addresses methodological issues arising from the international scaling-up of studies occurring in Cluster 1. Located at the top of the cluster are economists from the Netherlands, Australia, Sweden, and the US, who have contributed advances in the measurement of health equity. Near the centre of the network are a group of epidemiologists and social epidemiologists, many of whom have medical or sociology backgrounds. Both regions share an interest in the measurement of health equity, and the ways in which income and prevailing economic conditions affect health, including dental and oral health. Cluster 2 is around three-quarters the size of Cluster 3, but demonstrates 35% of that cluster's SNE diversity, reflecting the smaller number of disciplines represented.

Cluster 3: Health Disparities Research (US). Almost all members (96%) of the network's largest cluster are located in the US. One in every two researchers from Cluster 3 holds a PhD or other doctoral degree in Public health (including epidemiology) or medicine. Nursing, psychology and sociology PhDs are well-represented within this cluster, but political science and the humanities are absent. While the majority of authors in Cluster 3 have written about ethnic and racial disparities in health, researchers in the rightmost region of this cluster (near Clusters 5 and 8) share a particular focus on racial and ethnic disparities in cancer outcomes. Researchers in the leftmost part of the cluster (toward the centre of the network) have a more mixed focus, for example, maternal and child health, sexual determinants, drug use, mental health, and allergies. The top-left corner of this cluster includes several highly-cited researchers from the Harvard School of Public Health, and University of California San Francisco. These researchers are distinctive within this cluster for their long-standing focus on the relationship between socioeconomic status and health, perhaps explaining their strong citation links with Clusters 1 and 2. Cluster 3 might be viewed as a microcosm of the whole network, with social epidemiology on the left hand side, and clinical (especially cancer) epidemiology on the right. The emergence of this cluster and its links with other clusters is discussed in more detail in Section 6.4.

Cluster 4: Policy-Focused & Critical Health Inequalities Research (UK).

Cluster 4 is unique within the network for its disciplinary diversity. 52% of members are located within the UK, 19% in Australia and 17% in Canada. While median entry to the field for authors was 2005, Cluster 4 generally developed alongside Cluster 1, throughout the 1980's and 1990's, as the strong citation links in Figure 10 indicate. Researchers in this cluster have the network's most diverse doctoral trainings, with an equivalent 13 equally-represented disciplines among just 42 members. The Integration of this cluster is *higher* than the integration of the network as a whole, suggesting not only that a large number of disciplines are present, but that these disciplines are themselves diverse. The network's humanities PhDs and political science PhDs are concentrated within this cluster and the social sciences are also well represented. These trainings are consistent with the research focus of the cluster on macro or 'upstream' determinants, including political and corporate determinants. Many researchers in this cluster conduct qualitative research, or have a theoretical emphasis in their work. This cluster covers topics such as health policy, lay knowledge, and evidence synthesis. Cluster 4 has strong citation links to Clusters 1, 7 and 6, but is sparsely linked with Cluster 3.

Cluster 5: Racial and Ethnic Disparities in Cancer: Administrative Reporting (US).

Cluster 5 is the network's smallest cluster and (like Cluster 8) is cancer-focused and located geographically within the US. Members of this cluster have co-authored highly-cited, national administrative cancer statistics reports which include cancer incidence and mortality for racial and ethnic subgroups (labelled with the keyword "health disparities" since 2002). This cluster has the network's lowest Integration Index, as nearly one third of members have medical training, and remaining members have statistical, biomedical, or public health backgrounds. Most members are affiliated with either the National Cancer Institute, National Cancer Society, Centers for Disease Control, or National Institutes of Health (NIH).

Cluster 6: Socio-Critical accounts of Work-Related and Migrant Health (US, Canada, Spain). Cluster 6 is a mix of researchers from Europe, Latin America and the US. Researchers in this cluster are linked via a joint focus on employment-related health disparities/inequalities, and migrant health. Several researchers are based in Barcelona or completed doctoral study in that city. An additional group are located at (or have passed through) the Wake Forest Department of Family Medicine. In terms of disciplinary training this is the most diverse of the small clusters (Clusters 5–8), containing a mix of researchers with medical, biomedical, family studies, health policy, psychology, and statistical backgrounds. The network's anthropology PhDs are also concentrated within this cluster.

Cluster 7: Geographical Approaches: Inequalities in Place & Space (UK). This relatively new, chiefly UK-based cluster contains the majority of the network's geography PhDs. Located at the right-hand margin of Cluster 4, these researchers share a focus on spatial and geographic inequalities, environmental justice, and neighbourhoods. These eight researchers entered the field of HIDR slightly later than researchers in other clusters, with a median first publication year of mid-2008.

Cluster 8: Breast Cancer Disparities (US). This small cluster is comprised chiefly of contributors to a single project, the Breast Cancer Health Disparities study (Slattery et al., 2014), many of whom are (or were once) based at the University of Louisville, Kentucky, or University of Utah. This densely connected set of co-investigators is connected to the main network primarily via citations to and from researchers at the H. Lee Moffitt Cancer Centre, in Florida. This is the least-diverse cluster in terms of disciplinary background according to both indices, with an equivalent 3.4 balanced disciplines. This reflects the concentration of researchers holding a PhD in Health Behaviour, Health Education and Health Promotion (consolidated within the Public Health Subject Category). Other members hold advanced degrees in statistics or biomedical science. This is the network's most recently established cluster, with members' median first publication in the field being mid-2011.

Figure 10 does not lend itself to concise summary, however, the findings above can be summarised visually to convey the general features of the network. See Figure 11, below.

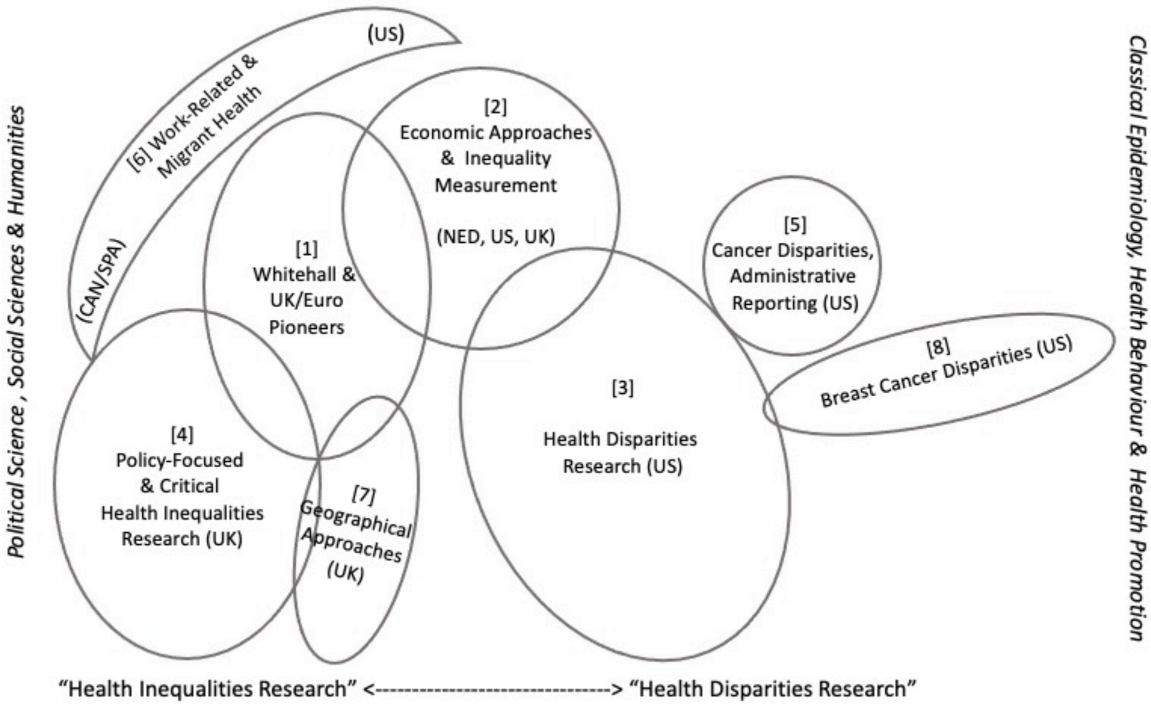

Figure 11 - Eight clusters of health equity research.

6.4 Exploring and explaining the eight research clusters

In this section, I draw on interview data to better understand how clusters have emerged and by what forces they are sustained in citation-space. As a reminder, I interviewed 43 network members and incorporated questions about an early version of Figure 10.

6.4.1 Landmark studies & advances in measurement

Major research projects have contributed to the form and disciplinary topology of Figure 10. An early milestone of health inequalities research was the Whitehall cohort, established in 1967 and analysed from 1978 onwards to investigate the relationship between cardiovascular (and other) diseases and occupational social class within the British civil service (Marmot, Shipley & Rose, 1984). The status of the Whitehall studies as paradigmatic examples of health equity scholarship is reflected in Figure 10, as Whitehall investigators and collaborators make up much of Cluster 1, and occupy a central position in the network.

Dutch researcher Johan Mackenbach spearheaded efforts to replicate Whitehall in Europe, beginning with the Dutch Longitudinal Study on Socio-Economic Health Differences, containing an explicit reference to Whitehall in its abstract (Mackenbach et al., 1994). Authors of similar single-country studies make up the top (left) half of Cluster 1. These replications provided comparable cohorts in several high-income countries and, therefore, the opportunity for cross-country comparison. This work was initiated by a group of economists, visible in Cluster 2. Throughout the late 1990's and early 2000's, the scaling-up of the field from single cohort studies to global mega-comparisons introduced methodological challenges, and an accompanying need for "valid measures and methods" (Manor et al., 1997). In response, a literature specific to the measurement of health equity emerged, largely authored by the economists in Cluster 2 and others located at the intersection of Clusters 1, 2 and 3 (Kakwani et al., 1997; Kawachi and Kennedy, 1997; Lynch & Kaplan, 1998).

In sum, in the UK and Europe, paradigmatic epidemiological studies aiming to investigate how social class (measured by employment status) impacts health played a key role in the development of Clusters 1 and 2. A shared focus on social inequalities in health outcomes facilitated links between these two clusters.

6.4.2 'Inequalities' & 'disparities'

The lack of citation links between Cluster 3 and the European/UK clusters is a conspicuous feature of figure 10. US researchers in figure 10 seem to mostly cite other US researchers, whereas researchers in Australia, Europe and Canada seem more interconnected. This may be a reflection of the specialised streams of research apparent in Figure 10, and specialised communities of (for example) geographers, economists, clinical epidemiologists or social-scientists, unaware of potentially relevant publications from other streams. Alternatively, these different terms may signify varied framings of health equity as biomedical or sociological phenomena. In interviews, most interviewees were unable to explain this feature of Figure 10, and interviewees in both network hemispheres commonly indicated they had little sense of the other, underlining their separation:

> I have no idea who anyone is! I've never heard of any of those people! […]. How is this all getting done in silos? And Why??
>
> Social Epidemiologist A (Epidemiology PhD), Cluster 3

> It is a bit hard to make sense of. There are a few names I've never heard of. I have to say […] I just can't think who these people are.
>
> Health Geographer (Geography PhD), Cluster 7

Some interviewees suggested space between Cluster 3 (US dominated) and other clusters is due to a combination of terminological differences and contrasting research foci, with UK based researchers generally studying *'inequalities'* between social classes, while researchers in the US tend to study *'disparities'* between racial and ethnic groups (Kawachi et al., 2002). However, this was only a partial explanation.

The separation between 'inequalities' and 'disparities' scholars in Figure 10 appears to reflect the distinct origins and independent development of two research traditions. Several inequalities scholars interviewed were keen to highlight the historical context of Cluster 1, in the wake of the Black Report (Black, Morris et al., 1980), the first systematic effort by any national government to understand and explain health inequalities between social classes (Smith, 2013). The 1980's and 1990's were periods of intense activity for British health inequalities scholars, as they attempted to address gaps in understanding identified by the Black Report, while documenting the health impact of policies put in place by the Thatcher-led Conservative government that had rejected the Report's conclusions (e.g. Whitehead, 1987). Many members of Cluster 1 and 4 pursued research on health inequalities throughout this period, despite limited funding, when the idea of health inequalities, even the term itself, was politically controversial:

> [It] was called the "health variations research program." We were told we couldn't use "inequalities" because Margaret Thatcher didn't like the term, so it was dumped and we were "variations".

Sociologist (Sociology PhD), Cluster 1

Interview data suggests this struggle contributed to a shared sense of identity among these scholars, now passed to some students and collaborators in Clusters 1, 4 and 7. There was resistance toward adopting any term other than ‘inequalities’ among these UK interviewees, because the term had been fought for by researchers perceived as pioneers:

> TC: Would you ever want to apply the term “Health Disparities” in the UK?
>
> I would rather we stuck with ‘health inequalities’. […] During the Thatcher time, you weren't allowed to talk about health ‘inequalities’, I don't like dodging away from it.
>
> Medical Sociologist (Social Science PhD)

There was also a sense that some UK interviewees considered ‘real’ health inequalities research as being concerned exclusively with social determinants, and restricted to network Clusters 1, 4 and 7:

> There is that community, and there are factions within that community […] But we would all be seen as ‘Health Inequalities’ […] Some [are] more on the periphery, like [Researcher from Cluster 7] for example because [s/he's] more geography. [S/he] is more on the periphery, but a part of the family. And then there are almost like ‘interlopers’ of the mainstream […] they probably think that they're health inequalities researchers, but [they don't belong to] this group of people who *are* health inequalities, who have carried that trajectory within them, and have been shaped [by], and learned from, those pioneers.
>
> Public Health researcher (Social Science PhD)

In contrast, in the US, the importance of social factors in determining health outcomes was catalysed by studies in the 1980’s noting differences in medical practice across apparently similar patient populations (McPherson et al., 1982). Responding to this unexplained variation in medical care, the US government commissioned the “Health, United States, 1983” report, which described, for the first time, significant differences in “the burden of death and illness experienced by blacks and other minority Americans as compared with the nation's population as a whole” (p.ix). The dominance of the term “health disparities” arose from this motivating drive to understand the ‘gaps’ in observed health outcomes and health care access between minority and majority ethnic populations (e.g. AMA CEDA, 1990).

This emphasis continued into the 21st century, with health disparities research in the US tending to focus on healthcare (e.g., Fiscella et al., 2000; Nelson, 2002), while the European concern with inequalities in health relating to social class also persisted (Marmot et al., 2012). Several interviewees noted the longstanding divide between scholars studying health inequalities (in class) and health disparities (in race):

> TC: Race is clearly very important to work on health disparities in the USA, it seems to be less of a focus in the UK?
>
> Yeah I've noticed that […] and I'm not sure why that is. [..] I haven't gone as deeply into it as I might, because it is not, to be quite blunt, it doesn't interest me that much, in the UK context.
>
> Health Policy Researcher (Social Science PhD), Cluster 4
>
> Here [in the US] it is very much on race, we don't talk about class here. […] There is so much focus on race here. Some of it makes sense, and some of it is really misguided and misses the point.
>
> Epidemiologist A (Epidemiology PhD), Cluster 8

US-based interviewees across the network, but especially in Clusters 3 and 6, expressed concern regarding the way race is conceptualized in research, particularly within 'mainstream' epidemiology. NIH requirements to report findings by racial and ethnic subgroupings were positioned as contributing to the uncritical treatment of race in quantitative analyses:

> Important now is to talk about race and ethnicity, to talk about race being a sociological concept and not a biological reality. […] In the US, because many of us are getting federal funds, one has to design research that covers human variation, and that is designated as sex, race and now age. […] This notion that you have to design it into your study means then that you have to be able to assign a value […]. In assigning that value then you have pretty much said "this is an entity, these different racial categories really are entities."
>
> Epidemiologist (Anthropology PhD), Cluster 6

In summary, in Figure 10 we see the lasting impact of the way research about health equity was conceptualised and initiated on either side of the Atlantic. Interview data reveal the ongoing importance of the way the two fields originated in the 1980's and developed over time, trajectories reflected in the structure of Figure 10. Perhaps more importantly, it is clear that 'inequalities' and 'disparities' are, in practice, not interchangeable terms for the same phenomena. To use one term aligns a project with a particular tradition of research, a group of pioneering investigators, and a historical conceptualisation of equity. Overlying geographic variation was disciplinary variation in use of these terms, discussed in the next section.

6.4.3 Disciplinary diversity

Doctoral trainings are not uniformly distributed across the network, mostly due to the mix of humanities, political-, life- and social-sciences on the network's left side (which, as Figure 2 illustrates, tend to study 'health inequalities'), and the dominance of medical, statistical, health promotion and epidemiological backgrounds on the right side (where 'health disparities' dominates as the preferred term). Researchers with economic training are similarly concentrated in Cluster 2, as are the majority of geographers (Cluster 7). The small, US dominated clusters (5 and 8) contain many more cancer epidemiologists and health promotion scholars than the wider network. Clusters 7 and 8 have the most recent median first publication date, and appear to represent regional communities of disciplinary and topical specialists. Clusters 1 and 3 (foundational clusters within the UK and US, respectively) have the same SNE diversity, though Cluster 1 might be considered more diverse, as it is smaller.

One notable anomaly is Cluster 6 (which includes a mix of 'health inequalities' and 'health disparities' researchers) appearing on the health inequalities 'side' of Figure 10 but does not include any members from the UK. Many members of Cluster 6 have social science backgrounds, and interviewees from this cluster explicitly framed the drivers of health inequity as socially-situated, which may explain the location of this group in citation space alongside clusters with strong social science membership. Nevertheless, interviewees in Clusters 1, 4 and 7 were almost universally unaware of the work proceeding in Cluster 6.

6.4.4. Disease focus

In addition to varied use of terminology, and different disciplinary profiles, the two network hemispheres differ in their disease foci, reflected in the algorithmic detection of two cancer-specific, US-dominated citation clusters (5 and 8) on the network's right side. This difference may be at least partially due to the data availability landscape within the US throughout the 1990's. In 2002, six members of Cluster 3 expressed their frustration that "few or no socioeconomic data exist in most US public health surveillance databases" (Krieger et al., 2002). In the context of this scarcity, the Surveillance, Epidemiology and End Results registry (SEER) held high-quality cancer incidence and outcome data alongside demographics dating back to 1973 (National Cancer Institute), representing a crucial data source for health disparities scholarship. Between 2000 and 2010, several members of Clusters 3, 5 and 8 utilised SEER to demonstrate racial disparities in cancer screening, incidence, and outcomes (e.g., Singh et al., 2004). Analysis of SEER data to investigate disparities in cancer-related outcomes was also utilised to demonstrate best-practice methodology (Harper et al., 2008). Cancer disparities research continues to be well-funded in the US, and is supported institutionally via Comprehensive Cancer Centres. Many interviewees in the US suggested that the security of cancer-specific funding powerfully shapes research about health disparities:

> TC: Many US researchers who have appeared in my bibliometric network study cancer. Why do you think that is?
>
> Because it's sexy. And well-funded.
>
> Epidemiologist A (Epidemiology PhD), Cluster 8

For interviewees in Clusters 3 and 8, there was a sense that studying cancer is a financial necessity, and that funding streams shape research questions. These data are presented (along with other references to research funding) in Chapter 7.

Partly as a reaction to the dominance of medical and disease-specific models in the US (Honjo, 2004), some US-based epidemiologists have advanced Social Epidemiology. These researchers, located in Cluster 3, helped cement social epidemiology as a mature sub-disciplinary specialisation (Galea and Link, 2013). Popular textbooks and key theoretical contributions to the field have been authored by members of Cluster 3, and the journal *Social Science and Medicine* (where many such contributions are published), is edited by members of Cluster 3. As these social epidemiologists are advancing a view of health as socially (rather than biomedically) situated, it is unsurprising that these scholars are located in the region of Cluster 3 closest to the social-science dominated network clusters.

6.5. Concluding discussion

Results presented in this chapter provide the first empirical test of the common assertion that HIDR is comprised of a broad mix of disciplinary trainings. However, this diversity is not distributed evenly across the field. Analysis of the network's 8 clusters reveals the presence of silos within HIDR, and a historical review of HIDR in the US and UK brings further nuance to the interpretation of the network's configuration.

Demographic and interview data suggest that, although disciplinary training played a role in the emergence of these clusters, so too have historical, geographic, institutional and financial (research funding) forces.

The appearance of social epidemiology as a distinct paradigm within mainstream epidemiology appears to be holding the two research communities together in citation-space, as this interdisciplinary specialty supports the interweaving of diverse perspectives. However, the 'bridge' researchers in Cluster 3 do not appear to have strong links to the critical, policy-focused strand of health inequalities research produced by authors in Cluster 4.

The ‘unexplored waters’ (Jasanoff, 2012) of health equity scholarship appear to lie in the gulf between (mainly) European scholars of the relationship between policy, health and social class and the (mainly) US scholars of the intersection between health and race/ethnicity. Despite strong links to the epidemiologically-driven Cluster 1, Cluster 3 is poorly connected to the social science-dominated Clusters 6, 7 and 4. Linguistic differences (‘inequalities’ vs ‘disparities’) do not fully explain this lack of connectivity, and rather themselves appear to reflect distinct, mature research traditions, each with their own history, disciplinary character, and funding landscape. US researchers outside the bibliometric network are studying the political economy of health inequity (e.g. Lynch, 2020) although, interestingly, some of this work relates to UK and European, not North-American policy (Lynch, 2017; Greer 2004).

Although these findings make clear that disciplinary training alone cannot explain the clusters comprising health equity research, disciplinary siloing is apparent. The paradigm as exemplary-past-achievement is also visible in Figure 10, as early framings of health equity in the 1980’s, in both the UK and US, have cast long temporal shadows and appear to have impacted the field’s structure and membership; in the UK, where health inequalities research was politically controversial, sociology and political science are well-represented, and now present established, independent traditions of health equity scholarship. In the US, where health disparities research began with unexplained variation in clinical practice, the clinical disciplines such as medicine, nursing, clinical epidemiology and psychology are more dominant, and ‘cancer disparities’ has emerged as a free-standing research domain, partly in response to independent funding streams.

It is possible that the clusters detected in this bibliometric analysis reflect deeper fragmentation in the conceptualisation of health, equity, and health equity. The following three chapters explore this possibility, with a focus on knowledge (Chapter 7), research methods (Chapter 8) and interpretation of statistical results (Chapter 9).

Chapter 7: Knowledge, Knowing & Epistemic Culture in HIDR

> [Science] is not one enterprise but many, a whole landscape - or market - of independent epistemic monopolies producing vastly different products.
> Epistemic Cultures (Karin Knorr-Cetina, 1999:p4)

> If you put a psychologist and an epidemiologist and a medical doctor and a sociologist in a room […] They have different models of the world, and how inequalities are produced.
>
> Health Equity Researcher (Sociology PhD)

The previous chapter detailed the diversity of disciplinary backgrounds within HIDR, and the uneven distribution of those backgrounds across citation-space. Boundaries were drawn around eight clusters, and certain disciplines found concentrated within those clusters. In this chapter I draw on interview data to investigate the extent to which network members hold different views regarding the generation and evaluation of knowledge about health equity. That is; whether HIDR contains diverse epistemic cultures, diverse 'strategies and policies of knowing' (Knorr-Cetina, 1999), and the extent to which this diversity appears connected to disciplinary training. The following analysis suggests there is meaningful diversity in the kind of knowledge researchers seek, and also in the criteria used to evaluate knowledge. But, what might be summarised as 'disciplinary tension' is more accurately described as a connected set of tensions relating to various elements of the disciplinary matrix: the type of knowledge researchers value, the structures via which knowledge is configured, the extent to which a discipline is strongly- or weakly-classified, and tolerance for complexity.

7.1 Knowledge and the 'Big Picture'

Researchers must choose which parts of the world are studied and which 'signs' are considered worthy of analysis (Knorr-Cetina, 1999; Latour & Woolgar, 1986). Eighteen interviewees mentioned the importance of the 'big picture', 'whole picture' or 'real story' when making these decisions, and this notion of a holistic view seemed to play an important part in deciding and justifying what should be studied, and how. There was widespread agreement among interviewees that good research moves beyond surface-level observation to connect with a ‘big picture’. However, interviewees were not in agreement about what the big picture is or how it can be analytically accessed, suggesting meaningful diversity in both what and how researchers seek to know.

Disciplinary training seems to play a part in setting up the view considered ‘big’, which in turn seems to shape the choices researchers perceive as ‘natural’ or ‘normal’ in scientific practice. Illustrative quotations are contained in Table 10, including references to the big picture as regarding causal processes and analytical approaches. These extracts also introduce the diverse research objects considered relevant in the study of health equity, and hint at the diverse conceptions of health as a social, physiological, behavioural and technical research object, to be explored in detail in this chapter.

	Big Picture	Illustrative Quotation	Interviewee
Sense of causal chain	The Dynamics Within Society	***My initial training in history*** *really enables me to take a* ***big picture [view]****. “What's going on here? Why? What are the different forces?” […] I was initially trained in a discipline that encourages, that suggests a focus to look at* ***the dynamics in the society***	Public Health Researcher (Social Science PhD)
	A combination of societal and physiological causes	*I am really signed up to the view that the whole life-course epidemiology, that* ***the conditions in which people live****, love, whatever, work and play, are important, but ultimately there are* ***physiological mechanisms.*** *So we have to look at both of them to get a* ***holistic picture****.*	Public Health researcher & Medical Doctor (Medicine PhD)
	Health Behaviour	*We* ***are very focused on intrapersonal ideas and concepts.*** *Things that are specific to the individual.*	Health Behaviour Researcher (Health Behaviour PhD)
Sense of analytical approach	A Comprehensive Statistical Analysis	***The whole picture****, kind of looking at various things. The size of the effect […] and confidence intervals are important [...] it's important to know what proportion of cases you could prevent.*	Biostatistician A (Biostatistics PhD)
	Use of theory	*When I read good theoretical work I think it contributes a new way of thinking about something [...] helping you understand* ***the bigger picture****,* ***the bigger story.***	Health Policy Researcher (Geography PhD)

TABLE 10 Interviewees referring to the ‘Big Picture‘

Regarding health equity, there seem to be diverse pictures which include biological processes, behaviour and 'interpersonal factors', and dynamics in society. In Table 10, one interviewee describes accessing the big picture via an appropriately designed quantitative study, while another suggests that, for them, a theoretical lens performs a similar function. Together, these data suggest variation in the following dimensions:

1. Researchers' sense of the causal processes relevant to the study of health equity, and

2. Attitudes about the points along these causal chains at which research effort should focus (i.e., the perceived location of authoritative evidence)

Interviewees were directly asked about (1), usually phrased as:

> "I am interested in understanding the mental models we carry around, as researchers. What is your back-of-the-envelope model of the process, or set of processes, that determine who is sick and who is well?"

I did not refer to 'health inequalities' or 'health disparities' in my questioning, as I wanted researchers to step back from the literature and their own work, to consider their most fundamental ideas about why health varies. I was also careful to avoid linking this question to interviewees' disciplinary training. Data concerning (2) emerged during discussions of what interviewees consider the hallmarks of 'good empirical work' (see Appendix A for the full interview schedule).

These epistemic commitments are connected, and appear to jointly reflect the type of knowledge researchers value, and aim to generate. Four distinct types of health-related knowledge emerged from interview data; knowledge about society, knowledge about disease, knowledge about behaviour, and what Knorr-Cetina (1999) termed 'negative knowledge', knowledge about how to get knowledge. These knowledges appear to be pursued for different purposes, and to be associated with what I term key scientific virtues. These knowledges, purposes, and virtues will be introduced and explored in this chapter, which concludes with a discussion of whether they suggest the presence of distinct epistemic cultures, and how these factors may have shaped the bibliometric network. The exception is the key virtue associated with negative knowledge, explored in the next chapter.

7.1.1 Knowledge about society

There was a clear connection between exposure to the social and political sciences and a social perspective on health. This social framing seemed like common-sense for many social scientists, as this interviewee explained:

> [Most important are the] social, political, economic processes which affect all sorts of aspects about people's trajectories in life, including, of course their health. […] Geographers […] what we think a lot about is those integrative processes that work across different scales, so it's a very natural way to work as a geographer, but I would say the same is almost equally true across all social science disciplines.
>
> Health Geographer (Geography PhD)

References to what feels 'natural' will be a recurring theme in every results chapter. Repeatedly, common-sense was invoked by interviewees of various disciplines to explain a variety of scientific practices. Barnes (1982) and Bloor (2011) note that appeals to common-sense within science very often signify culturally-determined standards, and what is presented as 'natural' or 'proper' should be treated analytically as that which is rooted in place and purpose, *communally judged* to be natural and proper (see Barnes, 1982: p.29).

Indeed, a social framing of health was not 'natural' for everybody. Social structure was identified as the leading, most important, or ultimate cause of ill health by 15 interviewees, and of these, 13 are located in the region of the citation network highlighted in Figure 12, below (four of the five researchers who mentioned politics or policy are also located in these highlighted clusters).

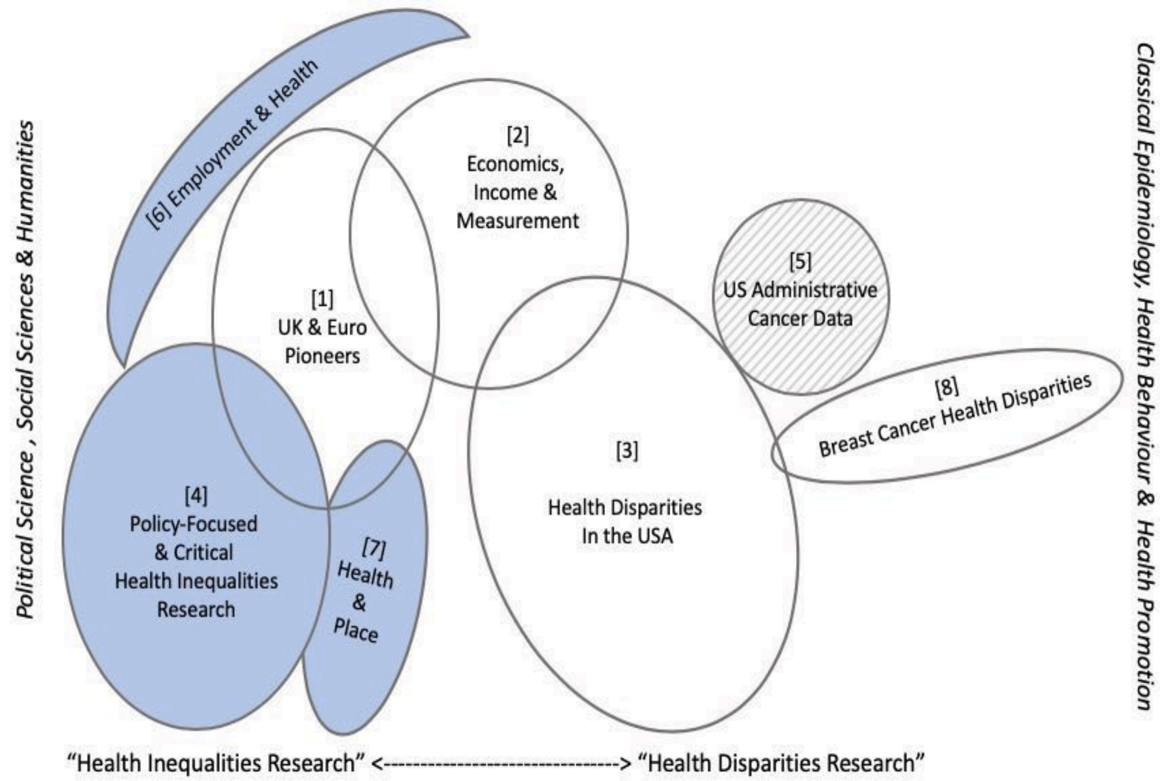

Figure 12 - Thirteen (out of fifteen) Researchers who specifically mentioned Social Structure as the ultimate or most important cause of health inequalities were located in network clusters 4, 7 and 6.

The conclusion that social scientists tend to think about health in a sociological way is not especially interesting. However, it is not only the case that these researchers view health as being socially-situated; for some interviewees, the underlying purpose and motivation for studying health is to better understand society:

> I see health as being a fascinating vehicle for understanding broader issues of social justice.
>
> Professor of Health Geography, (Geography PhD)
>
> Health inequalities, [is] an important way of framing what is happening to people, a way of understanding what is happening to people.
>
> Professor of Public Health (Health Services Research PhD)

Particularly important is that these interviewees presented *understanding* as the goal of their research. While improving population health is a shared goal for HIDR scholars, for many interviewees in the highlighted region of Figure 12 this improvement is understood to be achievable primarily (sometimes only) via an understanding of how society works, and how social structures impact health. For this reason, good research goes beyond the biomedical to encompass the social, because social factors and forces are where authoritative evidence about the drivers of health are understood to reside. This is not to suggest that other interviewees discounted societal factors as unimportant for health, but, in the context of widespread agreement that the social determinants of health are important, what appears to vary is the extent to which the sociality of health is incorporated into research questions, is approached directly via research methods, and is presented as an object of dedicated empirical interest. Disciplinary training was an obvious driver of this variation. Formal training in or other exposure to social science appeared to be a precondition for the recognition of social structures and processes as *research objects* relevant to health.

Some interviewees discussed the clear connection between their training and their thinking in response to my question about their 'mental model' of the process(es) driving inequality in health:

> This reflects my history background. People are really creatures of the structures in which they live. So every time, you look at historical circumstances. For me, I think the structural interpretation is the one that makes most sense.
> Public Health Researcher (Social Science PhD)

> Part of it is culture and part of it is structure, because I'm a sociologist and an anthropologist [laughing].
> Occupational health researcher (Anthropology PhD)

> My mental model is actually very geographical. […]Essentially, I think it is all about geography. Where you are, and where you grow up, and how you are, are all inextricably bound together.
> Public Health researcher (Geography PhD)

Historical circumstance, space, culture and structure are recognisable as research objects to these interviewees because their disciplinary training has familiarised them with a particular set of methods and style of scientific questioning. Not all interviewees had such clear correspondence between their training and their view of health, and these data are presented in Section 7.4.

Key Virtue - Reflecting Lived Experience

Four researchers from clusters four and six specifically mentioned that good research connects with or contains data relating to lived experience:

> Really good empirical work captures what it is like to suffer. It captures the pain of everyday life lived with enormous difficulty. That somehow stays with the 'public' in public health, that the people who are experiencing these difficulties, health difficulties that we're trying to ameliorate, have lives of enormous suffering […] it holds the people they're talking about in huge respect […] could almost have been in the lives they're describing. That to me would be fantastic empirical research […] continuing to say "this is what peoples' lives are like".
>
> Sociologist (Sociology PhD)

This view of good science as accurately reflecting lived realities consistently emerged in the transcripts of interviewees from Clusters four and six, which may explain the co-location of these geographically distant clusters in citation-space. For these researchers (mostly social scientists) the 'real story' of health inequality is the lived experience of individuals, and the impact of policy and social structures on that experience. This emphasis on lived experience aligns clearly with what Lamont (2009) termed the constructivist epistemological style.

Interviewees with training in geography were distinctive for their attitudes to complexity. Of the five researchers (Geography x2, Anthropology, Epidemiology and Social Policy PhDs) who identified research setting out to grapple with the complexity of social interrelationships as being especially valuable, four had some training in geography. Two extracts below from the same interviewee demonstrate a clear correspondence between their disciplinary training and attitudes about research quality:

> Social geography, and health geography within that, for a very long time has understood and written about the complexity and interconnection of everything.
>
> [Later in the interview]
>
> TC: What are the hallmarks of really good empirical work?
>
> P: That's a good question. That's a good question […] I think work which is

> oriented to understanding the complex and interacting nature of society, economy, culture and health.
>
> Public Health researcher (Geography PhD)

Studying complex social relationships feels natural and rewarding for this geographer, and this appears to have originated in their training. However, the study of complexity is not possible in all disciplinary paradigms, a point I explore later in the chapter.

In sum, within the region of the network highlighted in Figure 12, ‘good research’ explicitly engages with and describes the lives of individuals, the sociality of health and the impact of policy on health. This is a reflection of the view of social structures as the cause of health inequity, the recognition of these structures as objects amenable to empirical study, and of the motivating aim to better understand these structures via the study of health. These topics fall outside epidemiology’s problematique, and (as will frequently be the case throughout the thesis) understanding epidemiological norms and standards is key to understanding interviewees' attitudes, challenges and reported priorities, in diverse disciplines. Tensions between this view of health and other views are discussed in Section 7.2.1.5.

7.1.2 Biological Causes & Knowledge about disease

Very few interviewees stated explicitly that their research (or ‘good research’) relates to disease or biological outcomes. This probably reflects the dominant status of the epidemiological paradigm within HIDR, and the ways in which features of that paradigm are taken as given and do not require recapitulation or justification. One biostatistician made their preference for biomedical evidence explicit:

> The biomedical study and randomised study I would say are important [trails off]. How they are designed, I think they bring the best evidence.
>
> Biostatistician A (Biostatistics PhD)

Four epidemiologists and medical doctors indicated that studying the structural determinants of health does *not* provide the most desirable kind of evidence about health inequity:

> [I] don't mean to say that the fundamental determinants, like education, income, quality of education, family environment, community stuff, It doesn't mean to say that they're irrelevant, but it's a looooooong causal process from those things through to the actual manifestation of pathological disease.
>
> Epidemiologist & Medical Doctor (Medicine PhD)

> Ultimately, everything becomes expressed, at some level, in biological processes. Unless you are willing to deal with that, you're never going to really understand what the hell is going on.
>
> Epidemiologist A (Epidemiology PhD)

For these interviewees, due to the difficulty elucidating *causal links* between socio-structural and biological processes, it seems natural to focus research efforts on the proximal determinants, which are closer (in causal terms) to pathophysiology and disease, to where things 'become expressed'. Left unsaid is the status of knowledge about disease and ill-health as representing authoritative evidence about health equity, and the aim of learning about health equity via obtaining knowledge about disease. Although four interviewees made their *preference* for the study of disease explicit, many others discussed their work in ways which implied that the *underlying aim* is to generate knowledge about disease distributions and processes. One epidemiologist made that aim explicit:

> I am primarily concerned with preventing major diseases which may affect the health of people and cause premature death […] people are healthy, and people may become diseased, may get a disease, and the progression from healthy to diseased often depends on a number of risk factors for that disease, including the generalised risk factors of susceptibility. That is what I am interested in.
>
> Social Epidemiologist & Medical Doctor (Epidemiology PhD)

Improved understanding of disease processes was presented as important by interviewees in the network region highlighted in Figure 13, below (including the zones of overlap between Clusters 7, 4 and 1, and overlap between Clusters 1 and 2). This focus on disease stands in contrast to the kind of knowledge most valued by the interviewees highlighted in Figure 12, who generally foregrounded social causes of ill-health, and framed health as socially-situated.

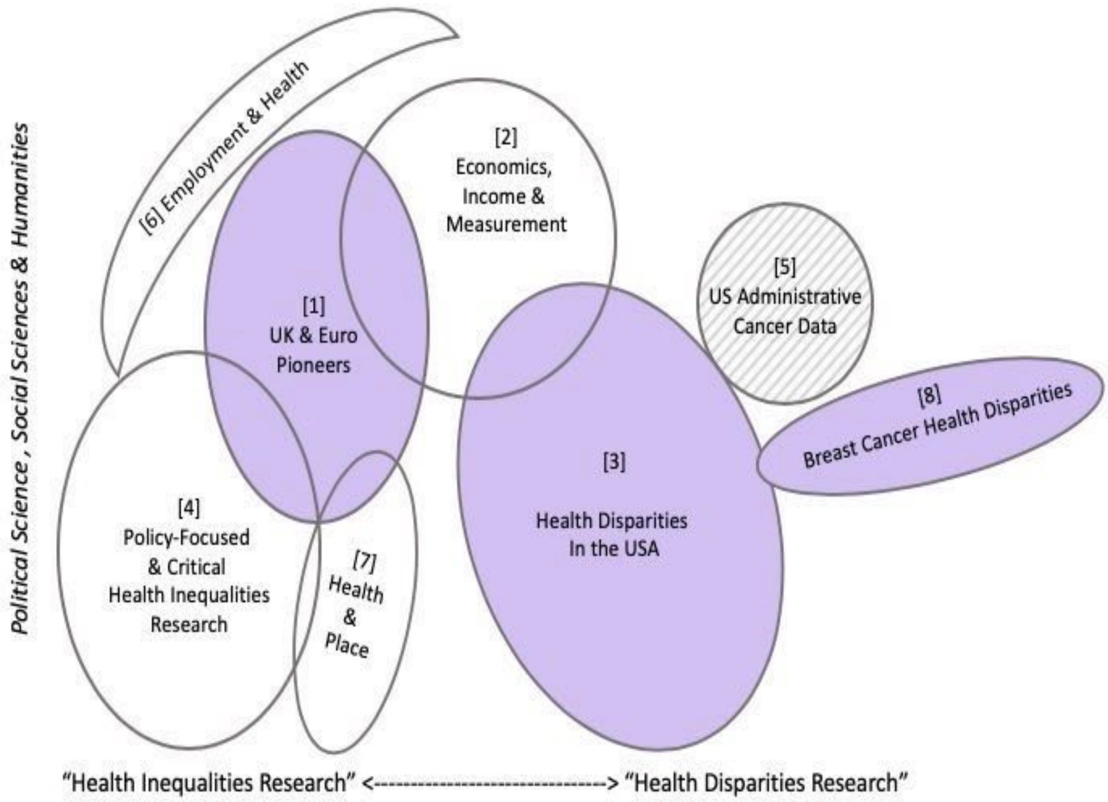

Figure 13 – Interviewees who foregrounded the importance of disease process and pathophysiology.

To generalise, interviewees in the highlighted region of Figure 13 seemed to value and seek knowledge regarding processes demonstrably driving pathophysiology within human bodies. These researchers did not discard or reject the social determinants of health, but the social determinants tend to enter the epistemology as 'risk factors' or 'exposures' alongside and in the same manner as behavioural or biological factors. These risk factors are then analysed with the aim of better understanding what causes disease, and it is the 'links' between exposures and biological outcomes which are the objects of principle scholarly focus:

> We are interested in the link between the exposures and the outcomes […] We need to know how it functions so that we can find ways to reduce it.
>
> Biostatistician A (Biostatistics PhD)

Apparent in the above extract is the motivation for studying exposures and their links with health outcomes: by better understanding disease processes, they might be avoided, interrupted, or minimised. This connects with the key virtue of this kind of research, generating useful knowledge.

Key Virtue: Useful Knowledge

Strongly-classified disciplines are characterised by a clear distinction between work which belongs to the discipline, and work which does not (see Section 3.4.2.1) Interviewees with epidemiological training expressed quite consistent ideas about good empirical work, across different sub-specialties of epidemiology, and different continents. These ideas tended to be collected under the banner of 'epidemiological rigour', described and discussed in the next chapter. Nine interviewees from the region highlighted in Figure 12 (whose work includes theoretical or conceptual elements from outside epidemiology) commented on the difficulty of gaining recognition from epidemiologists:

> Despite the fact that people like [Cluster 3 researcher] and [Cluster 1 researcher], some of the leading social epidemiologists who are shifting epidemiology, there is still a big core I think who are sticking to, sort of more conventional thinking about epidemiology, and looking for more linear relationships rather than looking at the complexity. And not wanting to look at the politics of the situation.
>
> Public Health Researcher (Social Science PhD)

However, one epidemiologist specifically highlighted this 'consistency in thinking' in epidemiology as a positive attribute, relative to the social sciences:

> [Epidemiology is] a very solid, clear discipline, and I like that.
>
> TC: Looking at the social sciences, is that less clear?
>
> Yeah, it is less clear. It doesn't have the same uniformity in statistics, and consistency in thinking.
>
> Social Epidemiologist (Epidemiology PhD)

Given this consistency experienced from within and without, epidemiology seems to be a strongly-classified science. Epidemiology is also an applied science. On that basis, drawing on existing analyses of the Biglan quadrants (Simpson, 2017; Becher & Trowler, 2001), criteria for evaluating knowledge in epidemiology are expected to be purposive or functional, or to reflect what Lamont (2009) termed the 'utilitarian' style. Purposive criteria for knowledge evaluation were apparent in almost all interviews with epidemiologists, and twelve interviewees with epidemiology training specifically reported being drawn to the subject because they wanted to do contribute something 'useful' , or practical:

> I wanted to do something, practical is a bit too much to say, but something that could potentially be relevant for making this world a better place. Not studying culture for the sake of more knowledge, but for the sake of making this world a better place.
>
> Public Health Researcher (Public Health PhD)
>
> [I did study] sociology, I felt I needed something more tangible. I wanted to feel that what I am doing has impact in the society, in humans. So it [epidemiology] seems to me to be very tangible, it is not abstract like the sociology discipline.
>
> Biostatistician B, (Biostatistics PhD)

Implied, but not discussed by these interviewees, was the corollary that abstract knowledge about culture or society cannot have the same impact, or be similarly relevant to making the world a better place. Generally, interviewees with epidemiology backgrounds displayed a preference for knowledge which can inform action, where 'action' was usually defined as reducing incidence of a specific disease. In short, in order to be 'good', epidemiological knowledge must be perceived as *useful* and as relating to *solutions*.

The desire to pursue solutions in epidemiology was repeatedly contrasted against the social sciences, especially sociology, by interviewees from both disciplines:

> I know I'm being utterly critical here, but sometimes I think "why do people get out of bed in the morning, if they're not going to do something that's going to be useful?" […] if your only goal is to contribute to theory, I

suppose that's an interesting problem, [that is] half-way to getting to a solution.

Public Health researcher & Medical Doctor (Medicine PhD)

I think epidemiology has, it comes from the medical field, very clearly. Fixing things is the ultimate goal. Sociology is much more a science which tries to understand society, the primary focus is not to fix society.

Health Equity Researcher (Sociology PhD)

The interviewee above who describes themselves as seeking 'useful' findings expressed a common viewpoint, that theoretical research appears to serve the researcher, by enhancing *their* understanding, and therefore has no clear purpose. But, 'useful' has different meanings in different research traditions. In the social sciences, improved understanding of social processes and patterns *is* useful. In epidemiology, useful typically means relating to solutions, and solutions are understood as being identifiable not via theoretical models, but via experimental interventions:

We want the evidence which will tell us what to do. It might not be evidence from interventions, but the evidence which would inform interventions.

Epidemiologist B (Epidemiology PhD)

'Doing interventions' emerged as a key distinction epidemiologists drew about their own work, and among themselves. One social epidemiologist suggested that their work is not 'helping', because it is not intervention-based:

It is appealing, that idea that I am helping promote change, but I am not doing interventions. Maybe one day I will do interventions but I'm not ready for that.

Social Epidemiologist A (Epidemiology PhD),

Interviewees also expressed a preference for interventions in the literature:

[Descriptively,] there are some areas which are untouched and we don't know what the size of inequalities is. But, of course, the emphasis should be on interventions.

Public Health Researcher (Public Health PhD)

And, interventions were presented as the 'natural' extension of descriptive research programs:

> I can take my research to the point of identifying barriers, understanding peoples' knowledge, but the natural next step would be an intervention.
>
> Health Behaviour Researcher (Health Behaviour PhD)

Scholars of the political and corporate determinants of health reported that the expectation of providing evidence-based solutions created challenges for the funding, evaluation and publication of their research. Generally, compared to the social and political sciences, understanding without an obvious path to action appears to be less-highly valued in epidemiology, where the aim, as one sociologist above expressed, is to 'fix', not to understand.

This drive toward solutions also shapes the kinds of questions epidemiologists ask and answer. One social epidemiologist described how the need to produce actionable knowledge diverted them from questions driven by curiosity, or a desire to understand:

> I could easily fall into 'wouldn't it be cool to look at' or 'what if we…' but I need to really think "what would we do with that? So what?" So, I think I am almost forced into that, but I love it. I really like that about being in a medical school.
>
> Social Epidemiologist A (Epidemiology PhD)

While no interviewee from any discipline argued that improving health is *un*important, epidemiology's purposive slant seems to functionally narrow the zone of legitimate enquiry to topics which can inform action. Here is a paradigm functioning in the way Kuhn (1962) describes: The epidemiological paradigm appears to restrict the kinds of questions interviewees considered legitimate, and also the form of acceptable problem-solutions. This tension between knowledge-for-action and knowledge-for-understanding gets to a question of deep epistemological significance: What is the purpose of scientific knowledge? I attend to the link between disciplines and research methods in the next chapter, but the above data shed light on an important aspect of disciplinary difference. While it is the case that interviewees from different disciplines study different *research objects* (social structure, disease risk-factors) this is one element of disciplinary difference, not its sum total. Researchers trained differently study different objects for specific reasons, entangled with the deepest commitments of their field (to improve health / to understand society). In interview data, what was presented by interviewees as tension or difference across 'disciplines' was, usually, more accurately described as a *connected set of tensions* across knowledges (pure and applied), collection codes (cumulative and integrative) classification strengths (strong and weak) and tolerance for complexity (high and low). Prototypical examples of these tension-sets are presented at the end of the chapter, and the impact of these tensions on interdisciplinary collaboration is explored in Chapter 10.

7.1.3 Behavioural Causes and Knowledge about Behaviour

No interviewee identified health behaviour (smoking, alcohol consumption, diet, physical activity etc) as an authoritative form of evidence regarding the causes of health inequity. However, a large amount of research output within HIDR relates to behaviour (see Section 1.4), and attitudes about health behaviour as a research focus permeated other discussions. Interviewees from clusters 1, 2, 3, 4, 6, 7 and 8 perceived a lack of balance within HIDR, and generally wished there was less research about behaviour, pointing variously toward funders, data availability constraints, and vested interests as tipping the balance in favour of behavioural studies.

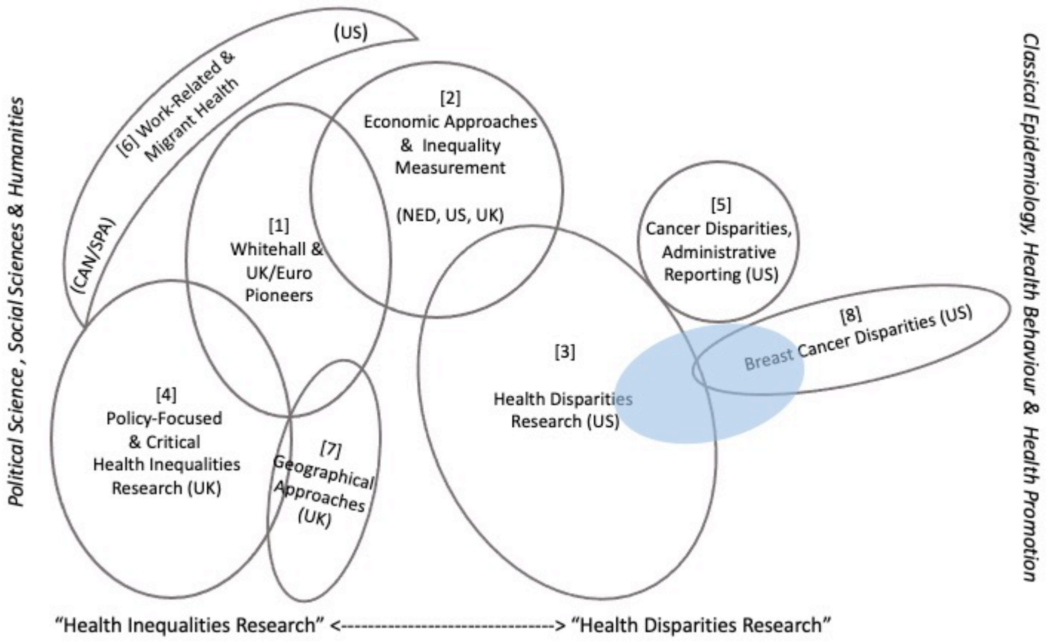

Figure 14 – Interviewees identifying as scholars of health behaviour

Four interviewees identified themselves as scholars of behaviour at the beginning of the interview. All four are located in the US, and work within the field of cancer prevention. In citation-space, they are similarly co-located, in the highlighted region of Figure 14, at the junction of clusters 3 and 8.

Just as social scientists viewed social forces as research objects of interest, and epidemiologists tended to focus on links between exposure and disease, behavioural scientists discussed aiming to study and improve health equity by studying and changing behaviour:

> The kinds of things we might be interested in, in health behaviour [come] from the perspective of really identifying the determinants of a particular behaviour. […] [and] trying to influence behaviour.
>
> Health Behaviour Researcher (Health Behaviour PhD)
>
> I bring in behavioural theories and methods, to develop interventions. Then I use the epidemiological principles of study design, etc, to design and test, to study the interventions that I develop. […] We've got to do more than just tell the public 'this is not good for you'.
>
> Cancer Researcher (Epidemiology PhD)

Knowledge about behaviour is therefore valued and sought empirically because it can be harnessed in the development of resources and programmes designed to encourage the public to behave in ways which minimise disease risk, particularly cancer risk. These interviewees do not want to understand behaviour for an academic purpose; the explicit motivation for studying behaviour is to change behaviour:

> I want it to be usable, not just 'here's what we did in this little study' […] to make it a better world.
>
> Nursing Researcher (Nursing PhD)

Conceptualising Behaviour: Tension between personal and published understandings

All four interviewees expressed a conceptualisation of behaviour as being intertwined with other factors, including social and biological factors. This holistic view of behaviour was presented as most accurately reflecting the world in which people live:

> Behaviour is a function of a person in his or her environment, given what they value. […] We've seen the iceberg image, where at the top we see the health outcome and [below is] what lies beneath. I think […] [that image] really can convey the essence of disparities.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

But two behavioural scientists reported struggling to achieve correspondence between this understanding of behaviour and the views contained in or suggested by their own published research. The conceptualisation of behaviour as a function of environment was positioned as conflicting with the individualist approach common within health promotion and cancer prevention. One interviewee specifically located that difficulty within their training, which appears to have again shaped what is 'natural':

> One thing we often overlook in our health behaviour world is that we are very focused on intrapersonal ideas and concepts. Things that are specific to the individual […] there is kind of a disconnect between the interventions or ideas that are proposed, [they] don't account for the 'real world' context in which things happen, or take place. I would say that's somewhat common.

TC: What do you think it is that's discouraging people from seeing those 'real-world' contexts?

I think the focus on the individual is a natural place to start, right? If you're thinking about behaviour change, you're usually thinking about it at an individual level. So a lot of our training […] our theoretical frameworks, they're often limited to intrapersonal factors […] sometimes we don't think about, kind of, the bigger context in which issues take place. […] [our] theories tend to drive us toward individual-level factors.

Health Behaviour Researcher (Health Behaviour PhD)

The social scientists from the highlighted area of Figure 12 patently *do not* consider it 'natural' to conceptualise behaviour as beginning and ending with individuals. These interviewees almost universally took a structural view of behaviour, and located its drivers within society. However, as the interviewee above explains, in the 'health behaviour world,' behaviour is more likely to be conceptualised as belonging to individuals, and this is positioned (in the above quotation) as being driven by the discipline's theoretical frameworks, by the health behaviour paradigm.

In addition to interviewees who identified themselves as scholars of behaviour, psychologists and economists described their disciplines as having an overt individual and behavioural focus.

Psychology is basically trying to understand individual behaviour. […] What psychology contributes is more the models for understanding how such behaviours arise […] [and] by giving input when it comes to designing interventions. Making sure that interventions are geared toward the sub-groups for which they are meant.

Epidemiologist B (Epidemiology PhD)

I would say that economics is becoming more empirical, in some sense it is moving closer to biostatistics, while also thinking about and understanding people's behaviour, and what things influence peoples' behaviour. […] We are probably less focused on outcomes [than epidemiologists], and are more focused on how you get people to do what you want them to do. Which doesn't sound very good [laughing].

Health Economist E (Economics PhD)

These data suggest that collaboration between health behaviourists, psychologists and economists may be more straightforward than for other disciplines (like sociology, or social epidemiology).

Perhaps surprisingly, some interviewees who took a structural view of health equity conducted research about health behaviour, but reported doing so against their better judgement, and in deference to financial or data-availability constraints:

> We collect data on behaviours, societal factors, also genetic data, so we can look at these […] [But] then, you get to the conflict between what you would like to do and what's realistic, what the funding allows. That is a different question.
>
> Biostatistician A (Biostatistics PhD)

> It's almost like there's this massive white elephant in the room that is the real [social] determinants [of health]. […] But it's like, we just ignore the 90% of what causes inequalities and focus on the one bit where we can get money and we can have control. I can see how that actually impacts on me […] even though you know, fundamentally, it will only make a tiny bit of difference, because that's where you've actually got the potential to do *something*.
>
> Public Health researcher (Social Science PhD)

I am not the first to suggest that research funding has epistemic consequences (Smith, 2010; Spurling, 2012; Lyall, 2019). But, these data reveal the specific ways population health researchers may bend (or suspend) their own epistemology in pursuit of funded research programs.

It struck me as an unlikely coincidence that all behavioural scholars of health equity interviewed were also interested in cancer prevention. As was mentioned in Chapter 6, I asked one North American interviewee to help me understand this focus, who explained that cancer is 'sexy, and well-funded'. One behavioural scientist framed the topic of cancer as a trap in which they have found themselves:

> We're pushed and funded to examine disparities in single disease conditions. And I am very guilty of this, I definitely need to have a funded research program […] And now I'm in this middle, like a hamster on this wheel. There is a lot of money to study cancer […] my papers and my grants are all focused in this area of cancer, but, transportation is an issue for getting your blood pressure checked, its an issue for getting your sugar checked, getting your glucose monitored. That effects everything. There are common underpinnings across a number of health behaviours […] Even

> though I may always maintain that way of thinking, my products maybe don't reflect that.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

Therefore, certain kinds of knowledge are perceived to be more fundable than others, with projects aiming to produce disease-specific knowledges appearing more fundable than cross-cutting projects. This may be because such projects can more precisely outline their 'usefulness', as understood within epidemiology. As an interviewee quoted earlier described, for researchers employing the biomedical model the 'loooooong causal process from those things through to the actual manifestation of pathological disease' may be viewed as a limitation, as a reason to direct research effort elsewhere.

A study with an individual, behavioural focus might therefore represent what is perceived could be feasibly funded, or studied, rather than what a researcher most wanted to study. Although many interviewees seemed aware of this pressure in their own work, this did not seem to dampen criticism of others. Research with a behavioural focus was the subject of fierce criticism, most especially by interviewees from Clusters 1 and 4, discussed alongside other inter-knowledge tensions in Section 2.5.1.

7.1.4 Negative Knowledge: Knowledge about getting knowledge

Six interviewees reported having no back-of-the-envelope causal model of health equity to report, and all six were trained in Economics or Biostatistics; disciplinary paradigms lacking a theoretical framework specifically describing the causes of disease. These interviewees appear to seek yet another form of knowledge, where the focus is not social, biological or behavioural, but *technical*. These researchers have chosen to spend their careers studying health equity, and so health equity is clearly of interest and considered important, but, when asked to discuss their own understandings of the processes generating health inequity, they did not do so. Instead, they emphasised technical aspects of the research process, especially data collection and study design. One professor of biostatistics repeatedly returned to study design when I asked about their mental model, and didn't seem to quite understand my question, or feel their own understanding had relevance:

> TC: You mentioned there that [as a statistician] you are a team-player on research projects. I am wondering how your personal understandings of health are expressed, in the work that you do?
>
> I think I've got what you are saying, but can you just reiterate your question, more specifically?
>
> TC: You pointed out that being a statistician is a bit different […] you might influence the design, but you don't always define the research question. Do you have your own mental model of what determines who is sick and who is well in the population? Or do you see yourself as implementing and investigating models that other people have?
>
> So, when you are young, it is hard to be determined to get the best study design […] I am a team player, and I am a statistician […] [When] the design the investigator or scientist proposes, when I find it's not optimal I am strongly against it […] It is, to me, morally illegal to have a bad study design. […] based on the scientific concepts, we need to try to get an optimal design.
>
> Biostatistician B (Biostatistics PhD), Cluster 3)

This interviewee repeatedly returned to design as the most important consideration in research, and discussed the importance of study design with conviction. Biostatisticians are experts in the design of clinical and epidemiological studies, and so the impact of this interviewee's training is quite clearly discernible here. Some economists also presented study design as a substitute for a global conceptual model:

> I suppose it is such a complex question, I wouldn't know where to start, to be honest. In some ways I don't think it matters too much about what influences health […] There are observational studies you can do, randomised controlled trials, there are all these different things that you can test out and try to think about what impacts on health.
>
> Health Economist (Economics PhD)

In clear contrast to the models of disease causation previously discussed, for the above interviewee the fundamental determinants of health are something a researcher 'tries to think about' by 'testing out' various designs. The implication seems to be that health is not firmly conceptualised as socially-situated, or as relating to disease processes, or behaviours. Rather, health is reflected in data, and the determinants of health are found not in society, or in the body, or in behaviour, but in well-designed empirical studies. This perspective was echoed within commitments to 'evidence' above any particular causal model, expressed by several interviewees in Cluster 2.

> I definitely have priors when I am predicting an equation, and I have beliefs about reducing disease and how you might reduce inequalities, but that is mainly based on external evidence, like the effectiveness of statins, and that sort of thing.
>
> TC: So it comes down to a series of questions about individual risk factors?
>
> Probably. Generally, yeah. Yes. [Pause]. I would say it is fairly empirical. I don't have a global… [trails off]
>
> TC: A global conceptual model?
>
> That's right.
>
> Professor of Health Economics B (Economics PhD)

In some scientific contexts, researchers seek knowledge which relates not to specific objects or phenomena, but to a better grasp of errors and more precise measurement. This *negative knowledge* (Knorr-Cetina, 1999) relates to the struggle of attempting to know, and provides

> "knowledge of the limits of knowing, of the mistakes we make in trying to know, of the things that interfere with our knowing." (p.64)

Researchers who have spent their careers trying to optimise the measurement or statistical handling of inequality in health do appear to be pursuing negative knowledge, with the purpose of improving other ('positive') knowledges. If health is an atomised collection of empirical findings, researchers can set aside debate about structural causes vs individual causes, and focus can shift to the technical challenges associated with the generation of empirical findings. Here again is the paradigm functioning as Kuhn described; the emergence of a specialised research focus ('quantitative measurement and description of inequality') liberates scientists from debates about fundamentals, and they can set off to solve the set of puzzles early work has presented. For many interviewees in Cluster 2 (including epidemiologists, medical doctors and economists) the optimisation and improvement of these technical aspects has become a dedicated research effort, with its own literature, and lessons. In citation space, the upper peninsula of Cluster 2 contains a number of economists who have played a part in the development of specific knowledge regarding the measurement of inequalities in health:

> We've made good progress on developing measures. […] We have learnt in the last decade that it [measuring health] is not the same as measuring income […]. Health is bounded at the top [by perfect health] and at the bottom [by death]. That gives us different characteristics. That is a lesson we learned.
>
> Professor of Health Economics C (Economics PhD)

This biostatistician seems to have developed specific, negative knowledge regarding the challenges and pitfalls of cluster-randomised trials:

> Based on my experience […] you collect your data, which is a tremendous challenge, [and] the true effect size is always different from what you planned, from what you expected. […] [Study investigators] have some imagination or idea about the effect size. "Yes, this group will be two times higher than this control arm". And it never happens! [laughing]
>
> Biostatistician B (Biostatistics PhD), Cluster 3

These same interviewees did not describe a global conceptual model of the causes of ill-health, perhaps because they do not perceive this as useful or relevant to the kind of knowledge they want to produce. This seems to support Kuhn's assertion that scientific tools (methods, conceptual models) are adopted only where they are understood to serve a relevant function (Kuhn, 1989). The development and uptake of tools within disciplines to serve particular functions is explored in detail in the next chapter.

The above extracts provide a glimpse of the way research in disciplines governed by cumulative codes can proceed as compartmentalised streams of questions. In both health economics and biostatistics, new findings bolt-on to existing knowledge in an additive manner, advancing the frontier as in a crystalline structure. In contrast, the social sciences are generally *integrative* in their collection codes (Becher & Trowler, 2001), meaning that at least some knowledge integrates, agglomerates and forms a mass which cannot be easily sub-divided, or decomposed. These disciplines operate at somewhat fuzzy 'frontier', which may be hard to locate, especially by those not trained in the field (Abbott, 2001). Under integrative codes, some findings cannot be comprehended (even when summarised) when isolated from underlying concepts and theories.

Health-focused research is an applied area of many social sciences, and this applied focus may introduce cumulative elements into the epistemology (more on this in Chapter 8), however the fundamental difference in the way knowledge accumulates in social and biomedical sciences may explain some persistent tensions within HIDR, most especially debate about the relative merits of 'decomposed causal associations' in HIDR, discussed in the next chapter.

Analysis so far suggests that interviewees are pursuing diverse kinds of knowledge, for varied purposes, and that these knowledges and purposes tend to broadly align with disciplinary background. The four knowledges presented above are summarised in Table 11, along with their apparent purpose, and associated conceptualisation of health.

Form of Knowledge	**Health is…**	**Purpose of Knowledge**
Knowledge about Society	A socially-situated phenomenon	Understand Society Change Society
Knowledge about Disease	A biomedical phenomenon	Understand Disease Incidence Reduce Disease Incidence
Knowledge about Behaviour	A Behavioural phenomenon	Change behaviour
Negative Knowledge	Empirical findings & technical challenges	Improve Other Knowledges

Table 11 Forms of knowledge about health

There was correspondence between interviewees' disciplinary training and the kind of knowledge they aim to produce in most, but not all cases. The question of how to manage causes at various levels (social, behavioural, biological) within a single study seemed to act as a refractive prism to demonstrate the way disciplines map onto the knowledges in Table 11. Twenty-two interviewees discussed the challenges of addressing multiple levels of causality in a single study. One epidemiologists and one medical doctor worried biological processes would be neglected or downplayed, six researchers from social epidemiology and the social sciences (especially sociology) worried that societal causes would be neglected.

One economist and one health behaviour researcher stated that this challenge is minimal, because their aim is to understand individual-level causes of ill-health:

> How do we individualise? How do we look at that individual, and not, because you know, when you get into public health you're looking at the population. But then [we need] to break it back down to the individual.
>
> Nursing Researcher (Nursing PhD)

Three geographers expressed worry about how varied conceptualisations of space would impact conclusions:

> Environment is not just the physical environment, for us [geographers] it is the social environment and the people you interact with […] Place and space for geographers is quite a broad definition […] [Other researchers] don't always think to analyse the individual experience in the wider context.
>
> Professor of Geography (Geography PhD)

One biostatistician framed their answer entirely in terms of the challenges associated with combining data sources, isolating the 'signal' from the 'noise':

> Yes the aggregate data, merging to the individual level is very challenging […] it is too much noise in the data […] that noise, it is at the individual level but also within the clusters, there is noise.[…] For me that is a challenge and it is hard to do validated and appealing research [with these data].
>
> Biostatistician B (Biostatistics PhD), Cluster 3

The above extracts, and Table 11, suggest the possibility of tension across knowledges and purposes. In the next section I present data relating to tension within and between knowledges.

7.2 Tensions & Epistemic Cultures

The rows and columns of Table 11 may represent academic distinctions, interesting to a sociologist of knowledge but of little consequence for research conduct. During interviews I took every opportunity to ask interviewees for details about tension they experienced across disciplinary lines, or to describe the 'disciplinary dynamic' of various situations they presented. On close examination, these related more precisely to the kinds of knowledge different disciplines tend to favour, to differences in disciplinary classification, and tolerance for complexity.

In this section I outline these tensions in detail, and ask whether the knowledges presented in the previous section (and tensions between them) signify the presence of distinct epistemic cultures, different 'strategies and policies of knowing' (Knorr-Cetina, 1994:p.4) which shape academic practice and underpin the generation of the 'vastly different products' referred to at the beginning of the chapter. This is the approach employed by Knorr-Cetina, using what she terms comparative optics to 'visibilise' the invisible, harnessing patterns detected in one domain as 'a sensor for identifying and mapping (equivalent, analogue, conflicting) patterns in the other' (*ibid*, p4).

Pure and Applied Knowledges: The 'Incomprehensible' and the 'Bizarre'

Work producing a different kind of knowledge to interviewees' own was frequently described as confusing, frustrating, or as being without scientific merit. For example, one psychologist trained in epidemiology reported confusion and frustration with the work of social theorists, who seem to produce un-intelligible research outputs:

> Part of me is quite impatient with some of the theorising that goes ahead. Especially in areas like sociology. I don't find it easy to understand much of it, and particularly when it comes to [topic], there are theorists who write things that are incomprehensible to anybody. And it's, kind of, is this helping? how is this helping? […] I guess I am more instrumental, I am more 'how can I use this?' 'how will it work for me, or for other people?'

Public Health Researcher (Health Services Research PhD)

One researcher's effort to describe or understand social phenomena is therefore labelled incomprehensible by another researcher, for whom it feels more natural to seek 'helpful' findings. This was presented as a disciplinary difference, but is a prototypical example of tension across pure and applied knowledges. This is also a tension across collection codes. For a researcher with experience in cumulative knowledge collection, research which proceeds in an integrative fashion may feel 'incomprehensible', because key foundational concepts are challenging to precisely define, yet tend to be central to analysis (Abbott, 2001). Compared to disciplines like epidemiology and medicine, findings in the social sciences may not be intended to be generalised, or very succinctly summarised (Lamont, 2009).

This tension between knowledge-for-understanding and knowledge-for-action appears to flow in both directions:

> As far as I can see, inequality research […] it has been a traditionally biomedical area. We get more funding than other parts of sociology [which creates tension for me]. Health is key to our society, and therefore it is more fundable than research on, let's say, attitudes, which is more core to the tradition of sociology.
>
> Sociologist (Sociology PhD)

This sociologist wants to study society by studying health. However, this creates professional difficulties within their sociology department, which prefers a focus on sociology's traditional concerns (i.e., not health). Sociology appears to exhibit local variation in its classification strength, and readers with sociology backgrounds may have different experiences within their own departments. But, at least some parts of sociology *are* strongly classified when it comes to the question of applied research, and more specialised writers than I have argued previously (in the terms used by the above interviewee) that knowledge does not flow from the 'periphery' to the 'core' of sociology as quickly, or as freely, as in the natural and medical sciences (Cole, 1994; Abbott, 2001). Four interviewees with training in sociology described difficulty publishing in sociology journals, and framed this as a ‘disciplinary’ tension, however the key issue seemed to be that the knowledge they were trying to disseminate was too applied:

> We have put qualitative research papers in journals with a more sociological focus, and they’ve always been rejected, I feel for really bizarre reasons […] there is also a lack of acceptance of applied health research, and applied research within those disciplines. […] [We] write what I think are quite sociological papers, but they are not seen as ‘sociological enough’ for some mainstream sociology journals.
>
> Professor of Population Health (Public Health PhD)

This suggests that applied knowledge is not highly valued within mainstream sociology, consistent with its classification as a ‘pure’ discipline in Biglan’s (1973) analysis. I do not know what ‘sociological enough’ looks like to mainstream journal editors, but a common thread running through interviews with sociologists was the importance of a coherent theoretical framework, and the desire to understand rather than to influence. This may explain the apparently ‘bizarre’ rejection of papers with applied foci.

The perception of these decisions as ‘bizarre’, and the description of theoretical knowledge as ‘incomprehensible’ seems to suggest the presence of quite different ways of knowing. It is not news that sociologists and medical scientists approach certain aspects of scholarship differently. What this analysis contributes is empirical confirmation of what Simpson (2017) suspected, that the dimensions identified by Biglan almost 50 years ago (Pure/Applied, Strongly/Weakly Classified) remain relevant, and capture something meaningful about disciplinary diversity. The tension between pure knowledges which enhance understanding, and applied knowledges which inform practice was clear in my data. Decomposing ‘disciplinary’ difference via these specific features may provide clarity in long-standing cross-disciplinary debates, and assist researchers to more precisely understand the difficulties they face in funding, conducting, and publishing inter- and trans-disciplinary work.

Classification and the nature of progress

The previous tension relates to the content of scientific output, but the same kind of mirrored tension was apparent on the topic of publication structure. Four epidemiologists reported feeling confused by the structure of social science papers, and framed this as a disciplinary difference, with sociology singled out for special criticism. One epidemiologist (choosing their words carefully) located their frustration in a perceived lack of efficiency, and lost scientific 'progress':

> I must confess that, personally, I feel that epidemiology and medical scientists are, in some way, quite efficient scientists. Very productive, and have clearly found a way to progress in our knowledge. [Long pause]
>
> I sometimes become a bit frustrated if I see researchers from the social sciences, especially quantitative research. […] There is, the way in which it is reported, with much emphasis on theory in the beginning, then brief results, no discussion. It is totally different to what we do, a brief discussion, a brief introduction, results and extensive evaluation. […] I think these sciences are much less able to make progress.

Social Epidemiologist (Epidemiology PhD)

The underlying issue here seems to be the way knowledge is presented in strong- and weakly-classified fields: In strongly-classified fields there is agreement about which questions are important, which methods are appropriate, and the form answers should take. As was discussed in Section 2.1.2, epidemiology seems strongly classified, and this may explain how it is possible (and common) for epidemiologists to publish works with relatively brief introductions. In the social sciences there is rarely such consensus, and a comparatively lengthier presentation of each project and its rationale is necessary. However, the volume of written material in the social sciences looks, to the epidemiologist above, like an 'inefficient' mode of scholarship, and the lengthy exposition in social science papers was described as overwhelming by epidemiologists:

> I don't have the stamina to read those long articles. I never did. I cannot train my brain to read a 20 page sociology [paper], I'm like "where are the equations??" ten pages of theory!? [Laughing]
>
> Social Epidemiologist A (Epidemiology PhD)

In epidemiology, economics and the biomedical sciences, published research most frequently takes the form of experimental reports, a genre "central to many conceptions of the sciences as empirical enquiry" (Bazerman, 1988:p.7). Whereas, in the social sciences, the structure of written articles is less uniform, and article lengths more variable. Such differences between scientific texts are "not just on the page" (*ibid*, p.16) but reflect the way the writer positions their work (and themselves) with respect to the social, textual, and natural worlds. Therefore, the existence of diverse academic genres in HIDR may itself be interpreted as signalling the presence of substantially different ways of knowing.

Integrative and cumulative collection codes (Bernstein, 1971) also inform article structure, and notions of scientific progress. In epidemiology, if new work is understood as positioned on the (common) frontier, no further rationale or explanation is required, and this manifests in extremely short introductions to peer-reviewed papers. In the social sciences, the frontier itself may require identification, justification and explanation, and time must be spent outlining the precise ways new work builds upon and braids together existing lines of enquiry, resulting in much longer introductory sections.

In addition, progress has a particular appearance depending on whether knowledge accumulates or integrates. As was discussed in Chapter 2, in disciplines with cumulative codes, old work is systematically devalued (relative to new work) old questions abandoned and replaced. Disciplines with integrative codes tend to return regularly to a central set of issues and motivating questions. Sociology has been repeatedly characterised in this way, and Abbott (2001) asserts that sociological knowledge is not progressive, but continually returning to the fundamental questions.

However, things are not as straightforward as the cumulative/integrative distinction might suggest. Abbott's (2001) discussion of fractal distinctions in academic culture cautions that, wherever such distinctions exist, there will be examples *of* both sides *on* both sides (social scientists working cumulatively, and epidemiologists working integratively). It also is not the case that 'progress' is obvious in epidemiology (where knowledge advances along a frontier) and difficult to discern in the social sciences (where knowledge integrates with what has come before), because these disciplines do not have shared definitions of 'new' and 'old', and this was evident in interviews (discussed shortly).

In classical epidemiology, the concept of external validity relates to whether findings can be appropriately generalised to different measures, persons, settings, and times (Steckler & McLeroy, 2008). If a finding cannot be generalised (usually because the sample is not considered representative) then repeating the same study with different participants, in a different place, or at a different time is a study answering a *new question.* However, to many interviewees, such work did not have the appearance of progress or seem to be tackling 'new questions'. Rather, this was framed as an example of the lack of progress in HIDR:

> There is just so much of the same old, same old. […] There are just a shedload of papers that go 'there are health inequalities' [or] 'we've still got health inequalities'.
>
> Public Health researcher (Geography PhD)
>
> Quite a lot of work in epidemiology is simply descriptive. Quite a lot of the work we publish is simply descriptive […] it's what's fundable. It's beavering away at the same old body counts, essentially.
>
> Health Policy Researcher (Social Science PhD)

This seemed to be reflecting a tension between HIDR and what some interviewees described as 'mainstream epidemiology' or 'the core' of epidemiology. There was a perception (especially, but not limited to social epidemiologists and social scientists) that questions about equity have been incorporated or absorbed into the wider epistemic machinery of epidemiological enquiry, and that this was incentivising the kind of research described in the two quotations above. I tentatively posit that this may be the case because, in the classical epidemiological epistemology, concern about the external validity of evidence can lead to the framing of inequalities in each new disease area, new geographic area, and new measure as a 'new question'. This intersects with the drive (in some settings) to maximise publication counts to favour the publication of small empirical advances, rather than research tackling bold questions. There was a perception that, in epidemiology, all that is required in the introduction to a publication is to demonstrate that the project described does not yet exist:

> There is a tendency in epidemiology, and social epidemiology, to be looking for the least publishable unit. And saying, 'people have looked at this, and this, but no one has looked at *this*', [saying] 'no one has made this table before'. And then [they] produce that table, without justifying why that table is of interest. I think that is a disgrace, I think that is polluting the scientific air.
>
> Health Equity Researcher (Sociology PhD) Cluster 1

One interviewee described this as the 'mainstreaming' of health inequalities research, and felt that this limits capacity to engage with the structural determinants of health, and normative origins of the field:

> Health inequalities has become a kind of everything and nothing. It has become embedded in things. It is a success, in that every grant proposal to the public health part of NIHR has to show how it addresses inequalities. That is an achievement, in many ways, for the health inequalities [research] community. But on the other hand it has de-radicalised what health inequalities really means. It becomes something like, "oh yeah, we stratified our analysis by socioeconomic status". Tick. Rather than thinking that, actually, there are these big, political, structural drivers [of health].
>
> Public Health researcher (Social Science PhD)

The format of epidemiological journal articles was discussed by some interviewees as actively limiting the kinds of things they can discuss, and the impression of dissatisfaction with academic genres cut both ways between epidemiologists and social scientists. Four social scientists expressed specific frustration with the lack of theoretical exposition in epidemiological studies:

> I find that in public health there is not the value placed on an exposition of your theoretical foundations, that there might be in anthropology or sociology. So I think when I read the results of research, I want it to be clear all the way through that, from the theoretical foundation, through the

> methods, to the results, that they all fit together. That's sort of cognitively pleasing […] I think sociology does that quite well.
>
> Epidemiologist (Anthropology PhD)

Intellectual coherence is presented by Becher (2001) as the defining scientific virtue of sociology's Biglan (pure, soft) quadrant, and theoretical coherence was raised as a specific marker of good work only by interviewees with sociology training. However, explicating a theoretical model is a challenge within the 3000-word limit typical in epidemiology journals (2500 words in some medical journals), and this was widely positioned as problematic by interviewees who pursue knowledge about society, and negative knowledge. Word limits were problematised in ways seeming to correspond with the form of knowledge interviewees pursue; this researcher with training in sociology wanted more space to set out their conceptual framework:

> The conceptualisation of inequalities is very important […] You have seen the papers in the medical journals, the introduction is two paragraphs! What about the conceptual framework? How can you understand the conceptualisation of the problem if you only have two paragraphs? […]
>
> TC: It is more difficult to publish work which has that conceptual framework?
>
> In the medical journals, [and] public health journals in general, it is very difficult.
>
> Public Health Researcher & Medical Doctor (Public Health PhD)

This economist wanted more space to fully present their statistical analyses:

> If I was publishing in public health or medicine I would probably try and lead with the simple stuff, and the complex stuff comes in an appendix […] If I was going in economics, it would be almost the reverse. […] [Public health] people think "you've bamboozled me with some complex model, I don't understand what's happening" […] in medicine and public health, you have 3000 words, people do not want four different sets of results, especially in the main text.
>
> Health Economist E (Economics PhD)

Medical and epidemiology journals have a long tradition of disseminating knowledge which fits within the medical and epidemiological paradigms, (which are more strongly classified than the social sciences) facilitating shorter article lengths. In addition to disease-focused knowledges being perceived as more fundable, disease-focused knowledge was presented as being more publishable, because epidemiology's strong classification facilitates dissemination in a format which may prevent other knowledges from being comprehensively presented.

Taken together, the above contrasts suggest differences in the ways knowledge about equity is pursued, and published. 'Progress' has different meanings, and takes different forms, reflected in the structure of scientific outputs.

Implied within the above quotations is the need interviewees felt to publish in epidemiology journals. Many interviewees, from varied backgrounds and seeking varied kinds of knowledge, discussed this necessity. Top epidemiology journals tend to have higher impact factors than journals specific to public health, social science perspectives on health, and health economics. If researchers are aiming to publish in epidemiology journals (for reasons related to impact-factor targets, and others discussed in the following two chapters), then the epistemological style and classification strength of their own discipline may be less relevant than what is usual within epidemiology. The dominance of epidemiology journals within HIDR may amplify the importance of epidemiological standards for research conduct, and effectively prevent researchers from asking questions which cannot be addressed using paradigmatic epidemiological methods.

One social epidemiologist reflected on why they have not pursued more qualitative work, and linked this directly to the possibility of publication in a high-impact epidemiological journal:

> It is not so rewarding, scientifically, to do qualitative research. It is much more time consuming, and you don't get into the International Journal of Epidemiology. The best you can hope is to get into Social Science and Medicine. So that is difficult.

Social Epidemiologist & Medical Doctor (Epidemiology PhD)

To my knowledge, the situation in which these health equity scholars find themselves is highly unusual within science. I am not aware of research from any other domain describing scholars working within one tradition (e.g., economics, social science, geography) being functionally obliged to submit their work for evaluation within another discipline (medicine, epidemiology).

The 3000 word manuscript for submission to an epidemiology journal appears to have become a key context within which research about health must be reported, and a 'social fact' to be reckoned with, as Bazerman described:

> "Though genre emerges out of contexts, it becomes the context for future works […] Now anyone with results to report must somehow address the context created by the social fact of this genre"
>
> (Bazerman, 1988:p.8)

Analysis suggests that a more comprehensive study of the way article formats and journal requirements shape knowledge construction in population health would be of value. In the next chapter I discuss how the 3000-word research publication also influences methodological choices inside and outside epidemiology.

In the quotation at the beginning of this chapter, Knorr-Cetina describes epistemic cultures as being 'independent epistemic monopolies', however, within HIDR (possibly within population health more broadly) they are *not* independent, because epidemiology's norms and standards exert force on the scientific practice of individuals working within a range of disciplinary frameworks. Epidemiology's strong classification promotes a particular picture of progress, a particular view of 'new questions', and the 'consistency in thinking' (as one interviewee put it) may narrow the zone of legitimate enquiry, creating challenges for researchers approaching the study of health equity from other perspectives.

'The World' and Tolerance for Complexity

Widespread desire within epidemiology to 'do interventions' was described in Section 7.1.2. This methodological preference reflects a deeper preference for simplicity over complexity in research findings, and preference for decomposed findings, extricable from other knowledge. These preferences varied across individuals, but also across disciplines in a predictable way, appearing to reflect what kind of knowledge is sought and valued.

Randomised interventions produce a particular kind of knowledge, a quantified estimate of the link between one exposure and one outcome, decomposed as far as is possible from the effect of other exposures. Compare this with one geographer's stated aim in Section 7.1.1, to explore the 'complexity and interconnection of everything'. Such an aim requires significant tolerance for complexity, not achievable within all paradigms. Four interviewees with training in geography criticised attempts from other fields to study geospatial exposures on this basis, as being too simplistic:

> If I see one more study of people looking at the distance from where people live to grocery stores and their shopping habits, their dietary intake, I will scream. Because that seems to be so simplistic and useless. […] the world is much more complicated.
>
> Epidemiologist (Anthropology PhD)

The below discussion of studies of neighbourhoods reinforced that, for some geographers, the aim is not understand space as an 'exposure' (as in a classic epidemiological study) but to understand the dynamics of social processes:

> If you you're thinking about neighbourhoods […] it's not just a simple question […] it is understanding the dynamics of the process that generates inequality […] all the things that feed into that, to produce unequal patterns, patterns and processes.
>
> Population Health Researcher (Public Health PhD)

Seven interviewees (six social scientists from the highlighted area of Figure 12) criticised the biomedical model on these specific grounds, as being poorly suited for the analysis of complex problems, and complex social processes. In section 7.1.2 interventions were presented as the 'natural' next step for both a research program and a career in epidemiology. But, this is not natural in all research domains, or for all determinants of health, especially where complexity is perceived and holistic study valued. Discussions of obesity were illustrative of the ways some disciplines value the decomposition of effects, whereas others prefer a holistic view which preserves inherent complexity:

> Obesity is an epidemic in the world, right? […] the balance should be put on the main causes […] the main things are related to the ways in which Big Food, the big corporations, are generating most of the food we consume. *That* is the thing, and so we should spend a lot of effort there, trying to understand *that*.
>
> Public Health researcher (Social Science PhD)

There is no randomised controlled trial of the ways in which corporations influence or determine the diets of individuals. The kind of work this interviewee wishes to see will generate understanding, not solutions. However not all researchers perceive the benefit of 'trying to understand', and the complexity which accompanies understanding obesity as happening *in the world* (as opposed to in human bodies, or in the behaviour of individuals) is not attractive to all researchers. One economist explained:

> What I mostly like a lot about economics is that we think in an abstract way, and try to solve a problem in a select way […] I like that we try to make a problem simple, so that we can solve it.
>
> Health Economist D (Economics PhD)

Simplicity is therefore a virtue in some epistemic cultures, reflected in the kind of knowledge which is sought and valued. In interview data, this preference for simplicity was connected to a preference for 'elegance' in research design among economists and some epidemiologists. When interviewees (from outside economics) described econometric studies as 'clever', what they especially seemed to like was the way these studies isolated variation in one variable, permitting the researcher to precisely estimate the impact of a single factor, treating all other factors as constant. The unspoken corollary of this is that engaging with multiple causes, or conveying real world complexity is *not* clever, or elegant. The balance between complexity and simplicity was also connected to methodological preference, explored in the next chapter.

Negative Knowledge? No thanks.

Negative knowledge did not escape similar tensions. Multiple interviewees described statisticians as 'strange', or 'different' and one erected a boundary between statisticians and 'normal people'. Whilst the contributions of statistical experts were widely valued, the pursuit of negative knowledge as a career choice seemed to be viewed by some interviewees as unappealing:

> When people say 'let's write a methods paper' - Ugh. No thanks! I'm not interested in that!
>
> Public Health researcher (Social Science PhD)
>
> Some of the statistical niceties […] None of that really matters to me. I'm sure it matters to some people. […] I'm not in that world, I'm in the world of "what can we do to tackle some of these problems?"
>
> Public Health researcher (Geography PhD)

This, too, cut in both directions. An economist with expertise in the measurement of inequality in health reported frustration with applied researchers from epidemiology and public health, specifically wishing they would focus more technical matters, and less on application:

> The measures I develop are used by public health researchers, and epidemiologists. But, how can I say this in a good way? They are mostly interested in applying these measures, and are not that much interested in discussing the underlying concepts. […] I try to explain that it is not just a cookbook, you should think very carefully about how you apply different inequality measures.
>
> Professor of Health Economics D (Economics PhD)

Here, again, is the epistemic drive toward (or away from) applied insight shaping research practice: one interviewee has little interest in the precise workings of a measurement tool, and another expressly desires more discussion of these 'underlying concepts'.

In interview data, and in the above economist's experience, interviewees with epidemiology training tended to ask 'how can I use this?', 'how will this help?' whereas biostatisticians, economists or epidemiologists with a strong interest in research methods (concerned with the pitfalls and challenges of knowing) might instead prefer to ask 'how does it work?' or 'is it appropriate?'

In all disciplines there is tension between negative and positive knowledges, between knowing-about-things and knowing-about-knowing, which plays out in research teams and research projects. Knorr-Cetina (1999) described the central importance of negative knowledge in High-Energy Physics, and contrasted this with Molecular Biology, and its overt focus on objects. But, in Knorr-Cetina's study, members of these two fields never met, and were not trying to collaborate. In multidisciplinary settings like HIDR, what appears to be tension across disciplines might reflect tension between the balance of negative and positive knowledges. For example, the two public health researchers quoted on the previous page view attention to 'statistical niceties' as an unnecessary break on the engine of knowing 'what to do' or 'how to tackle' inequalities. For the economist quoted on the previous page, concern about validity, bias and statistical power means this break is a crucial safeguard against erroneous conclusions, viewed by some statisticians and economists as the worst-case-scenario:

> [My stats people] say "this is what we can do, and this is what we can't". I go with that, because that is what they worry about, because they are statisticians and health economists and that's what matters to them, being methodologically pure. I need to feel confident that when I present the work some other stats person isn't going to [highlight flaws], and that's what they [my stats people] worry most about.
>
> Public Health researcher (Social Science PhD)

Tension between negative and positive knowledge does seems to have emerged on a grand scale in two long-running debates: first, the question of whether the association between health and income is causal, and second, the question of whether randomised trials are necessary in HIDR. Causality is discussed in Section 10.5.3, and the status of RCTs is discussed in Section 8.3.2.

'Interlopers'& knowledge about Behaviour

The above tensions were dwarfed in volume and intensity by the criticism levelled at knowledge about behaviour, and the researchers who seek it. Studying behaviour seems to represent a flashpoint for conflicting epistemic commitments in HIDR, and a topic where the moral dimension of inequality emerges for discussion and debate. Seeking to follow Latour and Woolgar's (1986) example of avoiding 'muck-raking', I present these data as efficiently as possible, and try not to dwell on sensational language or personal comments.

Five interviewees (from the social sciences and social epidemiology) stated that knowledge about behaviour does not usefully explain systematic, social differences in health. Four other interviewees noted that the most important questions about behaviour relate to its socio-structural drivers, and lamented that health promotion and health behaviour researchers rarely seem to ask these questions (perhaps for reasons articulated by one health behaviour researcher in Section 7.1.3)

In interviews, research about behaviour was variously described as 'useless', 'rubbish', 'unimportant' and in one case, as 'terrorism' directed at materially-deprived communities. Criticism also extended to the researchers who study health behaviour, labelled 'crazy' by one interviewee.

Collectively, these comments reflect one hallmark of good science which was common across all disciplines and clusters: good work answers 'good' research questions, where 'good' means 'important' (discussed further in Chapter 10). Researchers who ask and answer questions about behaviour were viewed by some interviewees as focusing on unimportant questions, or the wrong questions. This reveals the path by which knowledge about behaviour can inflame *both* scholars of society, and scholars of disease, as studies of behaviour were presented as being unfit for both purposes.

If the goal of research is to understand social structure, analysis beginning and ending with individuals will not support or enhance that understanding, especially if individuals are not studied in a way which bears witness to or reflects their lived-experience:

> You are saying “these people are behaving badly”, that is what you end up saying. […] [You are not] even considering what purposes the so-called ‘bad’ health behaviours play in peoples’ lives.
>
> Professor of Public Health (Social Science PhD)

Additionally, among interviewees with a biomedical or disease-specific focus, some studies of behaviour were described as having no obvious application, an unforgivable flaw in an area so strongly motivated to produce actionable findings:

> The social behaviour people […] drinking more wine, people are more happy? Do we really need to know? And what is the implication? is it to get people to drink more wine? There are so many more important crises on the Earth. It makes me nuts!
>
> Biostatistician B, (Biostatistics PhD)
>
> These things [studies of behaviour, nutrition] are only important if they help us understand whether health can be improved or not.
>
> Public Health Researcher (Social Science PhD)

Research about behaviour was framed as the ‘easy’ option by some social scientists and social epidemiologists, and was framed as being driven by political and/or corporate interest by two social scientists from Cluster 4. This is consistent with the ‘big picture’ common to these interviewees, of social structure as driver of health, and of health as socially situated. Two interviewees went further, and expressed doubt that researchers who study behaviour or behavioural interventions would identify themselves as scholars of health equity:

> [There are] other people who are much more focused on interventions, ways of improving health that are less focused on structural or social determinants type models […] So, they would be interested in the nature and success of their intervention, and probably have something to say about health inequalities research, but I would imagine most of them wouldn't identify as thinking about inequalities-related issues.
>
> Health Geographer (Geography PhD)

> There are almost like interlopers of the mainstream […] 'it's all about individual health behaviours', *they* probably think that they're health inequalities researchers.
>
> Public Health researcher (Social Science PhD)

Perhaps due to the field's history and politicisation in the UK (discussed in Chapter 6), British scholars of health equity appeared to feel a strong sense of what it means to be a 'Health Inequalities Researcher'. But, health behaviour researchers in the US were fiercely passionate about health disparities, and talked admiringly of researchers from the UK, who they viewed as having immense credibility. Perhaps, if these North American behavioural scholars were able to engage the above two interviewees in dialogue, an exchange of significant benefit would take place. These interviewees may come to might feel as I did during interviews with these researchers; a sense of humility and slight embarrassment at having dismissed them as 'interlopers' in the health equity research space. One UK interviewee seemed to have arrived at this conclusion as a result of collaborative projects:

> I had a tendency, like quite a lot of people in health inequalities research, to dismiss anything that is about health behaviours as 'too downstream'. Whereas I now see work that focuses on unhealthy commodities and unhealthy commodity industries as cutting across all of those different things. There is downstream work which is about trying to change peoples' behaviours, which I don't think is ever a route to tackling heath inequalities, because of the wide structural conditions […] [But] I do see that it cuts across.
>
> Health Policy Researcher (Geography PhD)

This echoes a comment from a health behaviour scholar in Cluster 8, reproduced below (from Section 7.1.3):

> […] There are common underpinnings across a number of health behaviours, health conditions, health outcomes.

Health Education & Behaviour researcher (Health Promotion PhD)

It seems that these two interviewees (from opposite sides of the Atlantic, and opposite sides of the bibliometric network) are really not in disagreement about the drivers of inequity in health. However, these two researchers are working within different epistemic cultures. The second interviewee felt unable to publish research which reflects their 'upstream' understandings, because of the norms within the fields of health promotion and cancer prevention. The first interviewee, working within a department of social science, faces no such restriction, but does need to navigate the challenge of publishing or presenting this work in spaces dominated by medical and epidemiological perspectives.

Analysis from this chapter helps to explore this difference (and other such differences) with reference to terms more specific than 'norms'. Health promotion and cancer prevention are more strongly classified than the social and political sciences, and have lower tolerance for complexity. It is therefore unsurprising that, in disease-specific funding proposals or publications, the presentation of behaviour as enmeshed in socio-cultural and environmental causes is not met with enthusiastic acceptance. 'Behaviour' therefore means something different in these epistemic cultures. For social scientists, behaviour reflects social structure, and the complexity of lived experience. In medicine and classical epidemiology, behaviour is a proximal determinant of disease which enters the epistemology as a risk factor or exposure. In health promotion and cancer prevention, behaviour is the point in the causal chain at which disease (in particular bodies) can be prevented.

It is worth noting that three interviewees actively resisted the opportunity to discuss tension or to express criticism of other parts of the field. The perspective of the interviewee below was particularly refreshing:

> I am prepared to accept that there are different ways of looking at a problem. […] you can't say 'that is not a valid way to look at the problem' if somebody else thinks it is, and especially if its work which has been published and peer reviewed. To say it is "wrong", I think I would be stepping outside my comfort zone.
>
> Health Geographer (Geography PhD)

Geography has been described elsewhere as being inherently interdisciplinary, which may explain this open-mindedness (Skole, 2004). But, despite these few exceptions, it was not widely recognised that there are diverse accounts of what is 'valid' in science, and that these ideas might be a function of disciplinary background, exposure to individuals, ideas, and literatures across a career. This suggests that what Lamont (2009) termed *cognitive contextualisation*, the ability and willingness to recognise evaluative standards from other disciplines, is not the norm.

7.3 Discussion

The preceding analysis suggests different policies and strategies of knowing are applied in HIDR, which themselves reflect varied kinds and configurations of knowledge and different tolerances for complexity. These differences underlie and underpin observed 'disciplinary' differences in HIDR, and do appear to suggest different epistemic cultures, which do not map onto specific bibliometric clusters but rather *regions* of HIDR (visualised in Figures 12-15). The conclusion that diverse epistemic cultures are present in HIDR is reinforced by comments from interviewees that peer review is not always 'peer', especially for social scientists:

> The problem is that when you apply to [funding] bodies […] most often you are not getting peer review. In public health you are still getting reviewed by epidemiologists who don't really understand sociology or political science. […] that is still a great frustration […] some leading social epidemiologists are shifting epidemiology, [but] there is still a big core I think who are sticking to, sort of more conventional thinking about epidemiology, and looking for more linear relationships rather than looking at the complexity.
>
> Public Health Researcher (Social Science PhD)

This single comment almost captures my preceding analysis. Coming to understand sociology and political science is a multifaceted task, which includes coming to grips with an integrative collection code and weakly-classified science, perceiving value in complexity, and accepting the validity of projects which aim to understand rather than intervene. Each of these is challenging for researchers who have trained in strongly classified, cumulative, intervention-focused traditions, because disciplinary training appears to establish the features of scientific practice considered 'natural' or 'common-sense'. However, the relationship between disciplinary training and the dimensions of variation identified in this chapter is not deterministic, nor necessarily straightforward.

Complications for a deterministic model of disciplines and knowledges

Kuhn presented scientific training as rather authoritarian and dogmatic, and argued that because paradigms demand monopoly of explanation, multiple paradigms cannot easily coexist. However, some interviewees expressed mental models and preferences for forms of knowledge which appeared in tension with their disciplinary background, and drew upon material beyond their initial training. In all cases, these instances were explained as being the result of exposure to a specific literature, person, institution, or research question at a formative career stage. Some illustrative quotations are below, in Table 12.

Influence	Interviewee's PhD	Mental Model	Other Relevant Extract
Literature	Epidemiology	It depends entirely on the **context, what the process is there and then**. One could say underlying that might be more historical, or economic forces.	For me **the anthropological model** was incredibly valuable [...] the different explanations of general cause to ones [explaining] a particular event's cause is something I don't think my discipline epidemiology thinks a lot about.
Individual	Psychology	These are the questions; **what is the nature of society, what are its processes**, what kind of people is it creating, how do people get blunted or heightened?	I received an appointment in the school of, it was called the Department of [Blank], and that's where [Blank] was, and [Blank]. **They were probably the primary influences upon me**, that's when I went from positivist psychology to critical social theory.

Institution	Biostatistics	I am very much influenced by what I do [and] where I am, **I believe that really it is society** and, kind of, the influence, the external influences and environmental influences which affect whether we are more healthy or less healthy. […] **there is probably a very strong influence of [my institution], the culture here**.	N/A

Table 12 – Non-Disciplinary influences on interviewees' mental models

While this thesis aims to elucidate the general properties of disciplinary approaches to HIDR, and the salient regularities of disciplinary influence, discipline is not destiny. There is not necessarily a straight line between researchers' training and their practice across a multi-decade career. The extracts in Table 12 reveal some other factors which can shape fundamental understandings, and add empirical detail to Ruscio's (1987) claim that the expression of disciplinary templates or genotypes is mediated by factors in a researchers' environment; 'a subtle, intricate interaction with many nuances' occurs between an individual researcher, their discipline, and their institutional setting. Examining how the factors identified as important in this chapter manifest in particular contexts would be a potential extension of this PhD.

Tension between personal and disciplinary models of health can cause researchers to test or bend the limits of their disciplinary paradigm. While some interviewees enjoyed correspondence between their personal understanding and the understandings conveyed by their published research, very often, achieving this correspondence required researchers to reach outside the dominant epidemiological paradigm toward the social sciences, which sometimes created professional and interpersonal challenges:

> Really, it is living in two worlds; Social science, and medicine.
>
> TC: Is that difficult?
>
> Um, no. I find it enriching to do so. It is a bit difficult, only for getting understanding and acceptance from colleagues.

Social Epidemiologist & Medical Doctor (Epidemiology PhD)

One extreme example was the interviewee quoted below, who left epidemiology mid-career:

> I felt that I personally, and the field of health inequity, had reached the edge of what it can do within the public health frame […] so I moved out of the school of population health. I'm [now] in the school of [blank]. The discussions I follow now, and engage with, are much more political science, international relations, global governance questions, policy processes, which are not necessarily about health, but these are the big underpinning determinants of health. I find those much more satisfying.
>
> Health Equity Researcher (Epidemiology PhD)

The felt need to produce research resembling epidemiological normal science (or, research publishable in esteemed epidemiology journals) has already been discussed. This need was in turn driven by the pursuit of research funding, career maintenance and advancement. Funding structures shape the work researchers do, and the need to obtain funding can cause researchers to align their output more directly with 'mainstream' modes of knowledge production, overriding their own sense of which determinants of health are important.

Five interviewees nearing retirement found themselves looking back at their published works with a sense of disappointment, or concern they had focused on the wrong questions:

> When I was young I was struggling to get promoted to assistant, associate, full professor, and also getting tenure. The annual evaluation always counted how many first author papers you've had. Now, luckily I have been through all of those things, [but] now I am thinking, does my work truly have impact on health?
>
> Biostatistician B (Biostatistics PhD)

For other interviewees, moving into the health-specific domain of a discipline was a mechanism by which they were able to overcome tension between their training and their own views. For example, this anthropologist found several aspects of 'mainstream' anthropology frustrating and declared themselves a positivist:

> There are anthropologists, and people out there […] who believe that science is a "white male chauvinist hegemonic discourse, and that facts are fascist." […] That's why I don't do that kind of anthropology. […] I'm a positivist. I believe there is truth, and that we can find it. We should be testing hypotheses.
>
> Occupational health researcher (Anthropology PhD)

This researcher found a more comfortable 'home' at the intersection of anthropology and epidemiology, where knowledge is evaluated in a more purposive manner, and testing hypotheses is the norm. Some economists, sociologists and geographers also reported a dissatisfaction with the abstract or theoretical parts of their home disciplines. Combining that training with epidemiology was a path to applied research, which they viewed as having more impact*:*

> Initially what attracted me [to health economics] was, I studied economics but I didn't want to put all my effort into selling soup in a marketing department or something. I wanted a more noble goal, to further population health.
>
> Health Economist C (Economics PhD)

A small number of interviewees insisted that their disciplinary training was not relevant to their current research. However, elsewhere in the interview these same interviewees would highlight features of their work which clearly originated in their training. One interviewee asserted that they didn't "feel" like a political scientist, as they didn't publish in political science journals, or read political science literature. However, when discussing their research interests, this same interviewee described a focus on political choices and decisions within political systems, including trade agreements. For this interviewee, political decisions are *research objects*, but this was not viewed as connected to or a consequence of their training in political science. Researchers are not randomised into disciplines (we all pursue subjects which match our interests), but in my interview data, the only interviewees who discussed policy processes as research objects were trained in political or social sciences. In this way, the influence of disciplinary training can be at once obvious and invisible, highlighting the importance of comparative study designs in research about disciplines. The perception of value within particular objects and data sources emerges in the next chapter as a key mechanism by which disciplinary background influences research conduct.

Epistemic Cultures, Knowledge, Disciplines and Paradigms

Knorr-Cetina (1999) explicitly rejected the usefulness of disciplines and scientific specialties as helping to "captur[e] the strategies and policies of knowing that are not codified in textbooks but do inform expert practice" (p.2). However, it does not seem appropriate to frame disciplines (or the content of their textbooks) as being *unrelated* to the aim of understanding how researchers in a given field 'know what they know' (p.1). By moving away from the structural account of knowledge-creation usually implied by reference to academic disciplines, Knorr-Cetina highlighted the technical, social and symbolic dimensions of two disciplinary cultures. In this chapter, four kinds of knowledge were identified and discussed, each with different purposes, priorities and epistemic virtues. These corresponded with disciplinary identity in a fairly predictable but imperfect way, revealing the important influence of mentors and literatures on scholarly practice across a career. In Chapter 2 the disciplinary matrix was conceptualised as a set of dimensions along which disciplines may be expected to vary. Disciplinary classification, collection code, form of knowledge, and tolerance for complexity emerged as important differences in this chapter.

But, what of disciplinary paradigms, and their influence? Since almost no interviewee discussed the importance of the fallen 'giants' on whose shoulders they stand, is Kuhn's model a relic of a bygone era? Kuhn took as his main subject the strongly-classified discipline of physics, and it is in the strongly-classified disciplines (classical epidemiology, and economics) that the power of paradigmatic norms were most evident. Beliefs about what epidemiological journals will (or won't) publish contributed to a narrowing of the kinds of questions researchers felt they could ask and answer. Because researchers from multiple disciplines seek to publish in epidemiological journals, epidemiology's paradigmatic commitments to useful knowledge, and to a focus on the links between exposures and outcomes, can act as intellectual grind-stones, sheering away the theoretical, the complex, the structural, and other kinds of knowledge which don't sit easily within a cumulative structure. However, clear trends and preferences also emerged among interviewees with social science training; a desire to preserve and engage with complexity, interconnectivity and contextual detail, and the value of lived experience as a data-source.

Kuhn's second interpretation of the paradigm as exemplary past achievement comes into its own when we ask the obvious question: Why? Why are some disciplines strongly classified, and others weakly-classified? Why does knowledge accumulate in some sciences and integrate in others? Why do some disciplines seek to eliminate complexity, while others focus on trying to preserve, capture and understand complexity? In Knorr-Cetina's parlance, if epistemic cultures are the machineries of knowledge construction, by what process are these machines assembled, and by what criteria is their performance evaluated? It is in answering this question that Kuhn's late writings are of primary relevance, and the epistemic power of the paradigm as exemplary past achievement is demonstrated. This is the topic of the next chapter.

Chapter 8: Work Worth Doing, Method & Theory in HIDR

The previous chapter explored the kinds of knowledge interviewees appear to be seeking as they study health equity, revealing disciplinary differences in the kinds of knowledge researchers value, and the way this knowledge is structured. Epidemiology emerged as a dominant discipline, positioning the epidemiological paradigm as a force in the *agonistic field,* (Latour & Woolgar, 1986) within which knowledge claims about health equity are evaluated. In this chapter, I explore how interviewees obtain the knowledge they value; namely, how members of the different epistemic communities outlined in the previous chapter select and apply scientific tools. Beyond this descriptive appraisal, I analyse discussions of method through the lens of a metaphor suggested by Thomas Kuhn in his essay *"Possible Worlds in the History of Science"* (Kuhn, 2000a), to explore why disciplines gravitate toward certain methods, and theories.

In the previous chapter, interviewees expressed strong feelings about efforts to obtain knowledge they did not personally value. Approaches not aligning with interviewees' goals were sometimes dismissed as 'useless', or 'rubbish'. The general sense of what researchers feel is worth doing emerged as a strong driver of both methodological and topical preference. Ideas about what is worth doing also varied across disciplinary groups, suggesting that disciplinary training shapes these ideas in important ways. Like the knowledge-types explored in the previous chapter, 'work worth doing' is not the manifestation of a single idea, or single preference, but reflects a connected set of concepts and preferences, many of which are encountered during disciplinary training. For example, an interviewee who viewed health as socially-situated explains what they feel is worth doing:

> Any time you are looking at societal structures and processes, that is particularly worth doing. If you can identify aspects of living and working conditions that are associated, and lead to adverse health outcomes.
>
> Health Policy Researcher (Psychology PhD)

The influence of epidemiological norms and standards apparent in the previous chapter will be revisited in this chapter (and the next chapter). For interviewees working outside the norms of the epidemiological paradigm, researchers' own sense of what is worth doing was frequently presented as being less important than what they could *convince others* was worth doing, and 'others' usually meant clinical or epidemiological colleagues. Just as certain knowledges appeared less fundable than others in the previous chapter, this extended to the methods via which such knowledge is acquired. One interviewee reflected on the challenges of funding qualitative work:

> If you're clinically trained, or you're kind of quantitatively trained, I think it is harder for you to see utility of it [qualitative research]. […] funding panels are a diverse bunch of people from a range of different disciplines […] you always have to be able to convince people that, actually, something is worth doing. […] it's kind of quite difficult to get some of that stuff through funders sometimes. You always have to make a case for it.
>
> Population Health Researcher (Public Health PhD)

As was discussed in the previous chapter, certain knowledges tend to accompany certain disciplinary classifications, certain collection codes, certain preferences for pure and applied knowledge, and certain tolerances for complexity. These characteristics are not randomly distributed across disciplines, but are connected to, are even reflections of, disciplines' intellectual content.

Not all methods produce all knowledges, and not all knowledges are accessible via all methods. For this reason the separation of methods from knowledges is not straightforward. But, while the value of a certain type of knowledge is tacit and frequently left unsaid within scientific communities (Knorr-Cetina, 1999; Latour & Woolgar, 1986) methodological choices are highly visible, and require explicit justification in grant applications and within publications. Most PhD theses (including this one) have "Methods" chapters, but no thesis or paper I have read has a "Knowledge" section, wherein the author argues why generating knowledge about society (or disease distributions, or behaviour) is worth doing, from first-principles.

If scientists were required to build up each study from such bare foundations, progress would be impracticable, and so, within certain scientific cultures the value of particular kinds of knowledge is taken as given, left unsaid. This is one application of the term “paradigm”, as meaning *that which can be taken for granted* (Kuhn, 1970: esp. p.37). Becher and Trowler (2001) discuss taken-for-granted values, behaviour and attitudes within the scientific settings they observed, and refer to this as ‘disciplinary culture’. But this approach leaves the *content* of science unexamined. Scientific values and attitudes might have their origins in research, Bloor (2011, p.368) noted that the turning point in the development and professional consolidation of aerodynamics, an emerging discipline, came when

> "experimenters were learning what they could take for granted. […]. What was once strange was becoming familiar and part of predictable, daily experience.”

Similarly, by the end of the events detailed in Laboratory Life (Latour & Woolgar, 1986), the mass spectrometer, the product of colossal scholarly effort, became a piece of furniture, an unremarkable presence. Similarly, the hormone TRH transitioned from an entity whose existence was actively contested (and demonstrable only via large-scale experiments) to an unremarkable white powder, shipped between labs in postage satchels for investigation of other phenomena. Latour & Woolgar added important empirical detail to Kuhn’s view of scientific progress as leaning heavily on “deeply entrenched expectations” (Kuhn, 1970), and revealed what this means for the role of technology in knowledge construction. These assumptions and expectations have functional consequence, especially for the use of scientific tools, because:

> The decision to employ a particular piece of apparatus and to use it in a particular way carries an assumption that only certain sorts of circumstances will arise (Kuhn, 1970:p.65)

In other words, the paradigm is the (unremarkable) background throwing scientific anomaly into relief. In multi-disciplinary fields such as HIDR, that which is bright, and highlighted by one backdrop may be dull and hard to discern against another. For example, epidemiology is the study of populations, but individuals within populations are not typically conceptualised as being part of any social structure, as one epidemiologist explained:

> "Most of us don't even pay attention to individuals, just thinking of proportions of variables in datasets, which are faceless and abstract quantities, divorced from their humanity."
>
> Social Epidemiologist C (Epidemiology PhD)

Likewise, epidemiological training does not (usually) equip researchers with a vocabulary for describing policy-development or policy change, as one epidemiologist who subsequently left the field pointed out:

> TC: Are there any kinds of research about health inequalities that frustrate you?
>
> All the epidemiology [laughing]
>
> TC: [laughing] Is there anything you'd like to add, at the level of individual publications?
>
> Yeah, so you've got all the epidemiology, it is marvellous work, I am not discrediting the epidemiology, but you get to the end of the papers and here are all these 'policy recommendations' without any understanding of policy whatsoever! Again, it is the arrogance of the field, saying that having done this kind of study you can answer these 'policy questions', but you've not asked the 'policy question' to begin with.
>
> Health Equity Researcher (Epidemiology PhD)

This was reinforced by comments from researchers with epidemiological training, who felt ill-equipped in this regard:

> What I think I need to understand much better […] is about the process by which research turns into evidence, turns into ideas, then turns into social change. […] I work as part of a [blank] specialist team, we have very weak understandings of how to affect social change. We just do things [studies] because it is the way that people do them.
>
> Public Health Researcher (Health Services Research PhD)

Epidemiological normal science (doing things 'the way people do them') therefore comes up short in describing political and social change. Criticism of disciplines on the basis of what a paradigm does *not* contain was common in interviews. For example, psychology was criticised for its individualist focus by four interviewees:

> Psychology is a very individual science, they look at individuals and individual traits, individual behaviours, not only are the methodologies different but the whole outlook, psychologists have often great problems in understanding populations […] psychologists have great difficulties in even understanding what social structure is all about.
>
> Health Equity Researcher (Sociology PhD)

Psychology is not a discipline organised around the understanding of social structures. That discipline is sociology, in which the interviewee quoted above trained. Economics was criticised by 8 interviewees, for (among other things) not 'understanding disease' and for focusing on the mechanics of the market economy:

> A proper health economics would be about […] what a fiscal sector should be doing if it wants to be healthy [improve population health]. For me that would be health economics […], should be what health economists do, but it is not what they do.
>
> Public Health Researcher (Demography PhD)

Health economics is not a discipline which developed around questions about disease, and the question 'how could a differently-structured fiscal sector maximise health' is not at the heart of the economic tradition, or compatible with the individualist ontology of mainstream economics (Mäki, 2001).

The above critiques add to those discussed in the previous chapter (e.g., whereby geographers generally wanted more attention to place and space). In these critiques, we see the way that, in Kuhnian terms, researchers' own disciplinary backdrops colour perceptions of and engagement with work proceeding in other disciplines. These critiques present an opportunity to explore more deeply how and why scientific training shapes a researchers view of what is 'natural' or 'worth doing'. In order to move beyond generalities, in this chapter I focus on the selection and use of research methods and use of theory.

8.1 Special Tools & Special Functions

In "Possible Worlds in the History of Science", Kuhn (2000a) presents the development of specialised scientific methods and isolated, inward-looking scientific specialities as sources of interpersonal difficulty, but as essential for the acquisition of new knowledge and progress of science. Just as speciation gives rise to creatures "more and more closely adapted to a narrower and narrower biological niche[s]" (ibid., p.102), so too scientific specialisation gives rise to scientists adapted to narrower and narrower intellectual niches. Some animals have limbs particularly well-adapted for climbing trees, but this same feature can produce an awkward, bent-legged gait when walking upright. The consequences of speciation in science are likely similar, Kuhn argues. Epidemiology is very well adapted to its defining niche (the quantitative description of trends in disease incidence and prevalence) but may be poorly suited to the description of phenomena outside that niche, such as why policies change or don't change. When viewed from the vantage of a paradigm adapted for the niche into which epidemiology is straying, epidemiological approaches (such as the 'policy recommendations' mentioned above) are perceived as out-of-place, and inappropriate.

Pushing further in this ecological direction: in the physical world, animals acquire tools (beaks, claws, tails, feathers) for exquisitely particular functions relevant to survival within a habitat. Removed from the habitat, at a distance from that intended purpose, these same features are exotic, and useless. The same might be true in science; When divorced from their function, from the raw materials upon which they operate, scientific tools can appear ridiculous, pointless or counter-productive. Kuhn summarised this idea succinctly: in science, what we have are "special tools for special functions" (Kuhn, 2000a p.98).

This connection between form and function repays deep consideration in multi- and inter-disciplinary settings. On the one hand, the generation of a certain kind of knowledge necessitates a certain kind of analytical approach - special functions require special tools. This is the conventional view of research methods as 'surrounding' or 'supporting' an epistemology, and some interviewees described methods this way, as tools to be taken up or left aside on a study-by-study basis, as research questions require:

> In-depth interview, focus groups, surveys, logistic regression, they're all tools. They're just tools. One starts off with a question: What is your research question? Given this question, and given what we already know, what is the best approach? Hmm.
>
> Occupational health researcher (Anthropology PhD)

In this model, one begins with a research question and selects a tool for the task. But, like much already explored in this thesis, analysis suggests that the connection flows in both directions. The questions researchers ask are frequently restricted by the methods with which they are familiar (and/or, researchers may pursue particular questions because they provide an opportunity to apply methods with which they are familiar, and confident). Methodologies are epistemologies operationalised, not peripheral, interchangeable accessories.

Returning to Kuhn's ecological metaphor, biological specimens imply a biological niche within which they can flourish. When touring a museum, even the most musty, glass-encased insect is understood to have once crawled about, to have flourished in some environment (however remote). Similarly, each tool in a hardware store implies a task to be made simpler, implies a task *worth doing*. In the same way, the existence of special methods in science implies a special epistemological function to be performed, and an intellectual context within which that function is valued. Just as scientists can become a "symbol and carrier" (Knorr-Cetina, 1999: p.220) for the kind of science they do, research methods might be symbols and carriers for the questions they answer, and the knowledge to which they facilitate access.

This connection between research tools and the problems they render manageable is little discussed, but key to understanding multi- and interdisciplinary research fields, because, as Kuhn implied, and others have reinforced (Barnes, 1982; Bloor, 2011; MacKenzie, 1981) the researcher's standpoint matters. The backdrop to ones' own thinking, the questions, answers, and generalisations pre-supposed or taken for granted shape perceptions of unfamiliar methods, and methodological decisions. In addition, different views of the 'special functions' central to good research may explain why methodological tension and controversy are perceived differently across paradigmatic divides.

The most obvious example of this in interviews surrounded discussions of how to evaluate causation, which could itself have been the focus of a dedicated thesis chapter. Fourteen interviewees (mostly social scientists and social epidemiologists) lamented the extension (even dominance) of methods initially designed to evaluate the effectiveness of clinical treatments to the study of health equity, and highlighted what they perceive as the inappropriateness of such methods for studying social interventions or phenomena:

> That challenge is a central challenge. The whole issue of establishing causation. Some people think you can only establish causation with randomised controlled trials. Well, that's not establishing causation. Randomised controlled trials are very important in saying if 'Treatment A' works, if a vaccine is effective. Randomised controlled trials are very important. But that doesn't establish causation of saying 'Education is important for health'. Establishing that causation is very difficult, you're not going to do it by randomising people to get more or less education and following them for fifty years and seeing how their health goes. Obviously not.
>
> Epidemiologist B (Epidemiology PhD)

However, as was discussed in the previous chapter, within the classical epidemiological paradigm, interventions are positioned as part of a natural progression within an epidemiological career. Funding opportunities were described as also favouring RCTs in public health:

> I think there has been a big shove in public health to try to generate evidence through, preferably, randomised trials […] The funding landscape is set up to strongly favour that kind of evidence, which is why we continually fund interventions that fail to tackle inequalities.
>
> Public Health researcher (Geography PhD)

Only two interviewees, one economist and one clinician-epidemiologist, expressed the view that randomised (or, more realistically, very carefully designed quasi-experimental) studies are necessary to prove that the relationship between income and health is causal. The below quotation was illustrative of this position:

> We still cannot answer the question "So, what needs doing if you want to reduce health inequalities?" In order to be able to do that you need causal evidence. It is easy to show that there are lots of associations recurring everywhere, it is very much more difficult to break the linkage. For that, you need quasi-experimental evidence where you use natural experiments like policies that have been implemented or abolished, to see what effects they have had on the health of various strata in the population.
>
> Health Economist C (Economics PhD)

What can explain the same method being described as indispensable by one researcher, and 'obviously' inappropriate by another? In the next section I begin to sketch an answer, by exploring the fundamentals of scientific tools.

8.1.1 Recognition of tools as tools

The process via which scientific tools are recognised as valuable, and their selection and application considered 'worth doing' emerges as a topic of interest. Continuing my metaphor from the previous section, wandering around the hardware store, one assumes that the things displayed there are tools. On safari, one may not understand the use to which bright plumes and elongated necks are put, but (trusting that evolution eliminates useless or disadvantageous physiological quirks) we assume some hidden advantage. These assumptions of utility are acts of intellectual generosity, and interview data suggested that such generosity is rare in HIDR (perhaps, in research contexts broadly). The recognition of tools *as tools* seems itself to be mediated by disciplinary training (by the kind of knowledge researchers seek, the organisation of that knowledge, and for tolerance to diversity and complexity.)

In the absence of widespread willingness to assume *a priori* that a thing is a tool, the recognition of tools as tools is a precondition for their acceptance and use. Such recognition requires familiarity with a tool's mode of operation, but also with the task it has been designed to simplify. A rock is a 'tool' if one knows about coconuts, knows they contain nutrients, and knows that rocks, suitably employed, can smash them. Without this knowledge a rock is just a rock; useless, inedible, uninteresting. A regression equation is a tool if one knows about beta coefficients, values the kind of insight they represent, and knows that estimating the coefficient is possible given sufficient data. Without this auxiliary knowledge, the equation is a meaningless, uninteresting scribble; regression is 'useless', 'rubbish', and not worth doing.

8.1.2 Preconditions for Adoption of Research Methods

One widely-recognised outcome of disciplinary training is the provision of graduates with a 'toolbox' of methods, and within HIDR the expansion of the communal toolkit has been specifically presented as an important marker of collaborative progress (Garthwaite et al., 2016:p.467). However, a well-equipped toolshed is of no use without knowledge relating to how, where and *why* tools might be employed. Access to diverse scientific tools is useless without companion sense of the phenomena to which they may be applied, familiarity with the insights arising from the study of such phenomena, and belief that such insights are valuable. I summarise this triple-stranded precondition for the adoption of a research method in Figure 15, below:

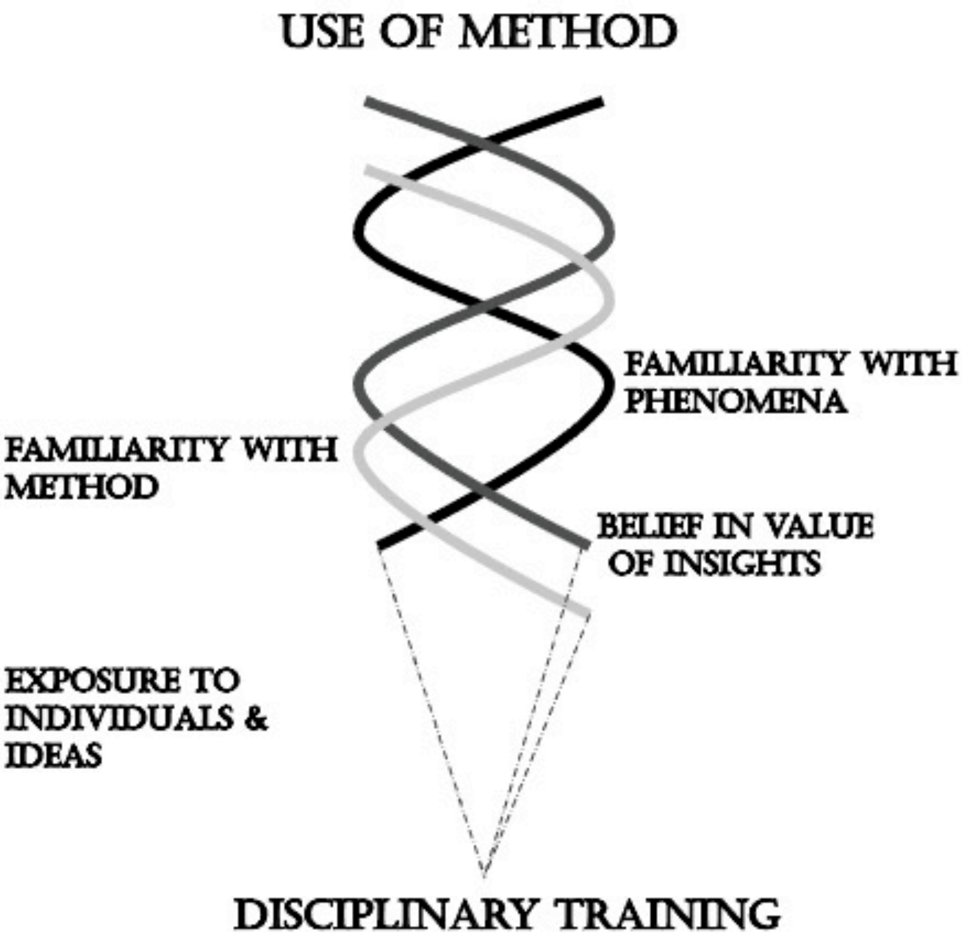

Figure 15: Preconditions for the adoption of a research method

Acquisition of a toolbox meets only one condition in Figure 15, exposure to method. In scientific settings, the presentation of a method purely in terms of its mechanics inevitably draws one question: Why? “Why would you do that?” The second and third strands of Figure 15 pose the answer; one uses a method to study phenomena pre-supposed to be interesting, to generate insight pre-supposed to be valuable. As was just discussed, however, this underlying justification is rarely made explicit. In interview data, the three strands of Figure 15 appear to collectively support a sense that a method is worth applying, that something is *worth doing*. What one interviewee described as ‘making the case’ for qualitative methods might be more specifically understood as elaborating the triple-stranded helix in Figure 15. Should one strand prove missing, the use of the method may be insupportable. This was most obvious in interviewees' discussion of qualitative research, and so I begin analysis there.

8.2 Qualitative Research

Twenty-two interviewees discussed qualitative methods in a positive or neutral light. Attitudes to qualitative work varied across disciplines, with researchers holding PhDs in sociology, social epidemiology, psychology and social policy being most likely to express positive views. Economists and classical epidemiologists expressed some scepticism or qualification, and two interviewees from these disciplines were overtly dismissive of qualitative approaches. Attitudes also varied across network clusters. In Clusters 4 and 6 (where health was generally framed as socially situated), qualitative research was presented as standing on its own merits as a research method. In other clusters, qualitative methods were more typically described and appraised as an addition or accessory to quantitative research.

It is important to note that the data in this section were generated in part because qualitative research requires justification in many public health research spaces. When one anthropologist began their discussion of qualitative research by saying:

> I am an expert in qualitative analysis, but I still see that as empirical science.
>
> Occupational health researcher (Anthropology PhD)

It occurred to me that an equivalent statement positioning quantitative research as "empirical science" is unnecessary (and was not expressed by any interviewee). Some interviewees reported that qualitative research faces an up-hill battle against charges of being 'unscientific', and that these charges sometimes extended to interviewees themselves:

> "Have you heard of [Blank]? Of course, the great guru of the RCT. He and I used to clash […] He would accuse me of being an 'unscientific qualitative researcher' […] we had a real battle over methodologies."
>
> Public Health Researcher (Social Science PhD)

There are no comparable data describing the struggles of quantitative researchers to be taken seriously or considered 'scientific'. The value of quantitative research appears self-evident. It is not the aim of this analysis to recapitulate commentary on the 'qualitative-quantitative divide'. But, as is reflected in Figure 15, my data suggest that one explanation for why qualitative methods are not always recognised as valid, free-standing scientific tools because not all researchers are familiar with these methods, and with empirical treatment of the phenomena they are designed to study. Additionally, not all researchers believe (or have previous experience suggesting) resulting insights are valuable. But first, how do researchers *with* this experience view qualitative methods?

8.2.1 Researchers who conduct qualitative research

Interviewees with first-hand experience employing qualitative methods were the most enthusiastic about their value. These were the social scientists in Clusters 4 and 6, and health promotion/ health behaviour researchers in Cluster 8. It was noted in the previous chapter that these groups focus on different research objects and have different research objectives. This was further reflected in attitudes to qualitative methods. In Cluster 4, qualitative methods represent an analytical window into phenomena of interest within the social sciences (e.g., policy processes and the lived experience of individuals). The six interviewees who identified qualitative methods as a hallmark of high-quality empirical work were all located in Cluster 4 and 6, and this seemed directly connected to the type of knowledge these methods access:

> TC: What are the hallmarks of really good empirical work?
>
> […] To me that is qualitative in nature, so, really good quality qualitative evidence that demonstrates that messy 'policy stuff'.
>
> Health Equity Researcher (Epidemiology PhD)

Here is Figure 15 in action. Qualitative methods are valued by the above interviewee because they grant access to important phenomena, to valuable insight. The three criteria for the adoption of a research method are apparently met; being familiar with the method, familiar with the phenomena it is designed to illuminate, and the belief that insight about those phenomena is of value. But, what are the specific phenomena which qualitative methods illuminate, and what is the value of the insights derived from qualitative analysis? In Kuhnian parlance, what are the 'special functions' which the special tools known as qualitative methods perform? Some clues are located within the five strengths of qualitative methods identified by interviewees who either conduct qualitative studies themselves, or closely collaborate with others who do:

I) Providing understanding of **lived experience**

> An understanding of the world from the perspective of people themselves. […] that is missing if you would rely purely on quantitative studies, because we do not have the perspective and the understanding of people themselves.
>
> Social Epidemiologist (Epidemiology PhD)

II) Leading to research **centred around and driven by research participants**

> [Quantitative research] is driven by you. It is not driven by the people in those communities. […] [In qualitative research] there is some shaping, but within that, it is actually fairly broad what can happen.
>
> Public Health researcher (Social Science PhD)

In the first extract above, 'perspectives' are understood to exist, as objects which can be studied, and they are presented as a valuable data source: the starting point for valuable insight about health equity. Qualitative methods therefore perform the special function of accessing these insights.

III) Qualitative methods provide a **more complete scientific picture**

> “A qualitative study would be a rich and novel kind of way of thinking about how people make sense of [blank] […] as far as I know, there has not been any work in that area and it would really enrich the picture.”
>
> Health Geographer (Geography PhD)

Similarly, for this geographer, understanding how people make sense of something is a worthwhile thing to do, and is considered relevant to research about health. Understandings exist, they can be studied, and are a valid and valuable component of the scientific picture. Qualitative methods are worth doing because they represent a strategy for accessing and learning about understandings.

IV) Qualitative work embraces and preserves **complexity**

> [I like when researchers] are using qualitative data for what I feel it really can offer, which is when I feel it’s really exciting, to help show the complexity which draws together various different theoretical insights […] it adds this depth and complexity.
>
> Health Policy Researcher (Geography PhD)

This researcher values complexity in research findings. Representing complexity is positioned as a useful thing to do, as an activity relevant to the study of health equity. Qualitative methods perform the special function of accessing and demonstrating complexity.

V) Brings research **closer to the ‘real world’**

> “How do we know in [region], [that] the salience of the measurement tools we were using was really missing the boat, until we talk to them and give them a chance to explain? It helps make our studies look more realistic and real-world, in their understanding and application.”
>
> Health Education & Behaviour researcher (Health Promotion PhD)

Here, qualitative methods represent an opportunity to improve the precision of measurement tools, to improve the applicability of research findings. Being ‘realistic’ entails better measurement, and qualitative methods perform the special function of providing an opportunity to check the suitability of selected measurement tools.

In short, qualitative methods perform the special function of generating detailed understandings of what life is like in certain places, at certain times, for certain people. The picture is made up of social objects, and the representation of these may be complex. Three researchers noted that some research questions are impossible to answer via quantitative methods, and that qualitative methods are a necessity in these cases. These questions related to policy and politics, and so qualitative methods also perform the special function of generating insight about these.

8.2.2 Quantifying Qualitative Data

The defence of qualitative research as being a part of 'empirical science' at the beginning of this section may have been a push against what some researchers described as qualitative purism; the belief that qualitative methods cannot and should not be combined with quantitative methods. The split between qualitative purists and non-purists was most evident in the discussion of Qualitative Comparative Analysis (QCA), the distillation of qualitative data into a series of cases represented by numeric variables. Once represented using numbers, logical inference is applied to determine the descriptive inferences supported by data. Some researchers found this innovation valuable:

> "We have been starting to explore QCA […] I think that has great promise when you want to bring some of that power that the numbers have. You end up with this richness of contextual understanding that you can start to look at in a quantitative sense. I think that is brilliant."
>
> Health Equity Researcher (Epidemiology PhD)
>
> QCA offers a way of blending that more quantitative approach, to most effectively categorise and utilise the stories and information that were shared.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

But two others (both social scientists, located in Cluster 4) viewed QCA as a troubling signal of a wider trend, the attempt to transform qualitative research into formats resembling quantitative research. Of particular concern to both interviewees was a sense that quantifying qualitative data destroyed what was perceived as most valuable about the method, destroying its special function:

> Qualitative researchers, some leading ones […] deformed the methodologies to conform to a mind-map that looks like 'quantitative research only different' in ways that ultimately undermine their legitimacy. […] Cutting up peoples' life experiences into snippets of a few words that can be coded, what does that have to do with peoples' lives?
>
> Health Policy Researcher (Social Science PhD)

For this researcher, knowledge about peoples' lives and lived experience is of significant scholarly value. Attempts to summarise or simplify this authoritative evidence-source are viewed as distortion (or worse), a perspective common in network Clusters 4 and 6. The above extract makes explicit a key question which seemed to underpin individual epistemologies from all network clusters:

> *'How comfortable am I with what I have discarded in the creation of my analytical product?'*

In Chapter 7, attitudes to abstraction varied across disciplines, and this corresponded with integrative collection codes (supporting and accommodating complexity) and cumulative collection codes (requiring at least some degree of simplification or abstraction). When discussing research methods, some epidemiologists and economists presented abstraction as a mechanism by which valuable insight is generated, and 'elegant design' seemed to act as shorthand for work which abstracts and simplifies.

Therefore, for some researchers, simplifying a problem is the first step in an analytical journey, not for frivolous or arbitrary reasons, but because it is a *pre-requisite for the application of paradigmatic methods*, for accessing insight communally-judged and pre-supposed to be of scientific value. In contrast, for researchers who believe good research captures the depth and complexity of social reality, very little detail can be comfortably discarded en-route to analytical insight, because complex reality *is* the analytical insight. For these researchers, simplification represents *a barrier to the application of paradigmatic methods.* All researchers who described good empirical work as capturing the reality of lived experience also mentioned qualitative methods as a hallmark of good research, and reported involvement in qualitative studies. This very strong correspondence between ideas about what research is for and how research should be conducted reveals the subterranean underpinnings of methodological preference, and represents another way in which the conceptualisation of methods as tools to be picked up and put down may be inadequate. Researchers' methodological preferences are connected to the kind of knowledge they consider valuable, which in turn is connected to wider purpose of research. My data bring contemporary support to the claim that researchers like the methods they like because they facilitate a kind of scientific enquiry perceived as important, ethical and useful (i.e. worth doing).

However this finding has (at least) one other interpretation. Rather than being motivated by a kind of knowledge and/or a particular sense of the goal of research, the causal arrow may run in the opposite direction (or in both directions). Aptitude in certain methods may shape the kind of knowledge researchers value. MacKenzie's (1981a) study of the controversy surrounding mendelian genetics presented the way researchers tended to value theories which allowed them to put their skills to good use. Similarly, in Bloor's (2011) study of aerodynamics, participants tended to dislike theories suggesting their skills are redundant, and tended to value the kind of knowledge produced by methods they are skilled in.

This PhD study cannot shed light on which interpretation applies more broadly in HIDR, or whether both apply. But, in either case, methods are not mere tools. In multi-disciplinary spaces, critique or dismissal of a research method may be experienced as critique or dismissal of the phenomena the method is design to illuminate, and the insight the method is designed to generate.

8.2.3 Researchers who don't conduct qualitative research

Eight researchers not directly involved in the conduct of qualitative studies discussed their attitudes to qualitative research during interview (during the activity described in Section 5.3, not in response to my question about the hallmarks of good empirical work). Among these researchers, qualitative methods were primarily valued for the ways in which they are understood to support or augment quantitative methods. Specifically, that they help researchers understand the mechanisms underlying statistical associations.

> I think it [qualitative research] is very useful, if you do a quasi-experiment, to then understand perhaps better what is lying behind it. I think it can be useful.
>
> Health Economist D (Economics PhD)

Here, qualitative methods are a means of improving or explaining results suggested by statistical causal inference procedures. Therefore, qualitative methods perform the special function of enhancing and improving insights arrived at via quantitative methods. Qualitative methods were also positioned as supporting the evaluation and development of experimental interventions:

> I think they have a place, both prior to and after things, to work out how you might improve things […] are there different ways we could have done things? To make things better? [I like] lots of process evaluation […] understanding these things can lead to better designs, even in health economics […]
>
> TC: Is that appreciation widespread in economics?
>
> No (Laughing)
> Health Economist E (Economics PhD)

> How do you know what intervention you should use? And what you should put in your intervention? Unless you do some qualitative work first, to understand?
>
> Cancer Researcher (Epidemiology PhD), Cluster 3

Qualitative insights have value in the two extracts above because they perform the function of generating insight about quantitative projects, facilitating improvements to the design and roll-out of subsequent projects (although acknowledgement of this value is reportedly limited in health economics). Unstated is the belief that the *truly valuable* insights are generated by an appropriately executed quantitative analysis. Such lukewarm support for qualitative methods was almost always tempered by an indication that qualitative research is not felt to be on the same level, scientifically, as quantitative research. No interviewee explicitly stated this view, however the implication or assumption frequently seemed to be that qualitative research conducted independently of quantitative research is of limited value, with no obvious 'purpose':

> It shouldn't replace the quasi-experiment. You need to do the quasi-experiment and then the qualitative research might help in trying to understand it better.
>
> Health Economist D (Economics PhD)

> I think, it has to be not done in isolation. If you do the qualitative for a purpose, to develop the intervention, then I think it tends to be more valued.
>
> Cancer Researcher (Epidemiology PhD)

This seemed to confirm the view expressed by qualitative researchers that their methods are undervalued within population health, and within some parts of HIDR. One UK-based sociologist described this as a consequence of the development of health inequalities research around quantitative findings:

> The field of inequalities was constructed around quantitative data […] The paradigms that dominate are epidemiological, and by definition they are statistical. […] The qualitative 'enriched' understanding of the quantitative, but the scientific status was accorded to the quantitative and not the qualitative. It was illustrative, it was decorative, it was secondary. It certainly wasn't central and on its own wouldn't constitute 'proof' of any kind.
>
> But, I think what has happened subsequent to that is, I wouldn't say they are of equal status but I think the power of qualitative and the necessity of qualitative research has been much more strongly recognised […] But, I still think for an awful lot of people who are 'the leaders' in public health research, the king-pin is still quantitative.
>
> Sociologist (Sociology PhD)

It therefore seems to be the case that some researchers view qualitative methods as indispensable, independent, and as generating the best sort of evidence. Others view these methods as being accessory to or evaluation of other methods. Overwhelmingly, the former are trained in a social or political science and the latter are trained in epidemiology, economics, or health promotion. But, as I outlined in the previous chapter, disciplinary tension is a connected set of tensions, and what appears to be a 'disciplinary difference' is more likely to be a connected set of differences.

As suggested by the three strands in Figure 15, interviewees with training in humanities, social sciences, policy studies and health geography were equipped with the terminology and theoretical frameworks to perceive and discuss the value of qualitative data. But, more than this, qualitative data sources represent the arena within which paradigmatic theories and concepts can be encountered. One sociologist described liking qualitative research because it illuminates "what structure and agency mean as dynamic and interlocking processes" and because it provides

> a kind of feeling for what structure-and-agency means. Not as abstract books, or conceptual framings, but what it means for the day-to-day lives of the people who are in the poorest circumstances, and the poorest health.
>
> Sociologist (Sociology PhD)

For this researcher, qualitative data provides a direct connection to concepts and theories fundamental to the discipline - qualitative data sources are the raw materials from which key insights are wrought. But 'Structure' and 'Agency' are not concepts which exist within the classical economic or epidemiological paradigms, and the description of them as an 'interlocking process' has no meaning within these research traditions. Qualitative data do not represent a way for researchers with epidemiological or economic training to encounter and explore *their* disciplines' central concepts or foundational theories. Qualitative data do not provide these researchers with examples of 'the familiar in the unfamiliar' (Barnes, 1982), they are simply unfamiliar. This is perhaps why researchers with epidemiological and economic backgrounds tended to perceive and evaluate qualitative research in terms of what it can do for quantitative research, because quantitative analysis is the setting within which the core concepts of these disciplines are encountered. This was reflected in the view expressed by some researchers that qualitative research does not provide 'answers':

> I probably instinctively prefer quant, because I like to have an answer, so that's an epistemological statement in itself.
>
> Public Health researcher (Social Science PhD)

Whereas others clearly felt qualitative studies do provide 'answers':

> That's where having a plurality of methods is really useful. I don't think you can necessarily answer some questions using quantitative data, but actually talking to people is the way to answer those questions.
>
> Population Health Researcher (Public Health PhD)

Of course, the capacity for a research method to deliver 'answers' depends on the question a researcher is asking. The role of research questioning will be explored in Chapter 10, but, for now, the qualitative-quantitative divide seems reflective of much more than simple methodological preference. It is a reflection of ideas about what it means to be empirical, what it means to get answers, and what it means to be scientific.

8.3 Quantitative Research Methods: Meaning, Artefact and Truth

In Chapter 7, knowledge about society was linked to the key virtue of reflecting lived experience, which also appears to drive methodological preference. Knowledge about disease was linked to the key virtue of 'being useful', and this epistemic force within epidemiology shaped diverse aspects of scientific practice, even for members outside the discipline. In this section, the key virtue associated with negative knowledge (knowledge-about-knowledge) is presented.

Broadly, concerns expressed by researchers with a technical focus signalled a preoccupation with bias, spurious results, and statistical artefact. Biostatistics is very closely connected to epidemiology (and might even be described as a subspecialty of epidemiology concerned with negative knowledge). It is therefore unsurprising that the focus on bias and artefact was widespread in epidemiology also.

Generally, for researchers with a technical focus, 'good' methods were described as those which most completely reduce the risk of artefacts, or guard most appropriately against likely sources of bias. "Avoiding bias" is a special function which quantitative methods are expected to perform, and the following analysis illustrates the importance of this function within epidemiology's natural (intellectual) habitat. However, underneath the discussion of artefact and bias is a feature of quantitative research which emerged from interview data and, to my knowledge, has not been highlighted elsewhere.

Clearly apparent was a sense that only some statistical results have meaning. In the absence of complete numerical data, distinguishing between statistical results which do and don't have meaning is the central challenge of quantitative research, and the possibility that results are meaningless can very rarely be definitively escaped. This constant threat of meaninglessness appears to shape the way economists, epidemiologists and biostatisticians select and evaluate *all* research methods. In other words, in these disciplines, *safeguarding against meaninglessness* is a special function which all scientific all tools are expected to perform[12].

Generally, though with exceptions, researchers with epidemiological training sought to escape meaninglessness via research involving precise

[12] A more comprehensive discussion of the ways interviewees pursue and verify meaning in statistical results can be found in Chapter 9

measurement and large sample sizes:

> We have to do work that is epidemiologically sound. We have to do work that includes a large enough sample size that we can address or examine the fact that we are looking at. We can't make generalisations, either with the results of the study or in the populations we are studying, unless the study is soundly designed and has a good sample size. So those epidemiology principles are drilled into my head.
>
> Cancer Researcher (Epidemiology PhD)

Frequently collected under the banner of ‘epidemiological rigour’ or ‘sound epidemiological design’, these attributes were explicitly positioned as methodological safeguards against error and bias:

> TC: What are the hallmarks of really good empirical work?
>
> […] I am a stickler for getting rid of sources of error. So I always look to see whether the confounding is taken care of, the measurement error is taken care of, and the selection bias is taken care of. For me, from where I sit, that is the hallmark of good research […] studies which look for causal estimates by getting rid of the three key systematic sources of error.
>
> Professor of Epidemiology (Epidemiology PhD)
>
> Good empirical work for me is a rigour in the methodology and the approach, so it’s all the things we’re taught in our research methods class, thinking about things like our study sample, how we’re sampling, are we using the best techniques when it comes to measuring our variables?
>
> Health Behaviour Researcher (Epidemiology PhD)

While the focus on randomisation, quasi-experimental designs and intervention studies was more common among epidemiologists specialising in cancer research (located in the US, in network clusters 3 and 8), concern about bias and small sample sizes was expressed by epidemiologists and biostatisticians across the network, including in Clusters 4, 6 and 7.

> TC: What are the hallmarks of good empirical research, for you?
>
> P: […] you quite often see small studies which really cannot answer the questions. So being a pedantic methodologist, I like people to think about sample size, whether the study is powered enough, which I think many people don't do. Biostatistician A (Biostatistics PhD)

Here, good research presents findings which emerge from an analytical process designed to minimise bias and systematic error. It is the *design* (the sample size, the precision of measurement, the ways in which error is accounted for) which confers legitimacy, relevance and meaning. This process is not compatible with all kinds of data, and cannot produce all kinds of insights, because such knowledge relates not to specific individuals but to populations, the defining epidemiological research object. Social structure and policy processes are not accessible via this epistemology, and were not presented as research objects or data sources by interviewees who pursue knowledge-about-knowledge.

As already discussed in Section 7.1.1, lived-experience was positioned as an authoritative source of evidence by several social scientists. For researchers pursuing knowledge-about-knowledge, authoritative evidence was obtainable via sufficiently large, well-designed studies. In an interesting echo of the interviewee who described accessing "depth" via qualitative methods, quantitative studies were described as moving beyond surface-level trends and revealing what is 'really' happening:

> From epidemiology […] the critical gaze, the kind of continual worry about confounding, that what we think is going on is not actually going on, and is driven by something else. Because I think that can be very useful, and I think there is an awful lot in social science and general social and political thinking […] there's not a lot of really asking yourself what's *really* going on here?
>
> Public Health researcher (Geography PhD)

Here we catch a glimpse of the epidemiologist's natural (intellectual) habitat. This 'continual worry' about confounding did not appear in epidemiology by chance, and is not sustained by accident or coincidence. Rather, it is a legacy of the crucible within which the discipline matured: the debate around smoking and lung cancer. Epidemiological results very rarely have a single interpretation (explored further in Chapter 9) and the discipline's history is riddled with false alarms due to failures to sample appropriately, or failure to adjust statistical results for a relevant factor. Systemic bias is the death-knell of an epidemiological study, and is potentially lurking in any component of a study's design; sampling, recruitment, measurement, and data analysis. As a result, designing an epidemiological study feels rather like moving tentatively across a thin sheet of ice; a single misstep can undermine an entire project, presenting a need for tools with quite particular functions. ‘Continual worry’ seems appropriate, large studies feel safer than small ones, and accurate measurement is imperative. Randomisation is understood as the best way to eliminate confounders and systematic biases, and it is for this reason that randomised studies sit near the top of epidemiology’s “Evidence Pyramid” (Petticrew & Roberts, 2003). But, these epistemological safeguards are adopted for their function within a specific intellectual context, and when moved outside that context, such focus on generalisability and bias becomes a source of difficulty and frustration for collaborators (see Chapter 10).

Three, specialised approaches to quantitative bias were apparent in interview data, belonging to economists, social epidemiologists, and epidemiologists embedded within community settings. To better understand these approaches, I explore the particular ‘special functions’ of quantitative methods valued by these three groups of interviewees.

3.4.1 Economics

To provide some context for the data presented below, at the time of interview, all interviewees with economic training were located in either continental Europe, the US or Australia, and were therefore working outside the UK research context. There are several health economists studying health inequalities in the UK, however it is the group in Cluster 2 (not based in the UK) who appeared in the bibliometric network, likely due to their highly-cited early research in the specific area of inequality measurement. A comparison of views among economists in different research contexts would be an interesting extension of this project, but is not my aim. All interviewees with economic training were based within departments of economics or health economics, not departments of epidemiology or public health, perhaps reflecting the insular, strongly-classified character of this discipline, and confirming existing descriptions of economics as ’insular’

(Fourcade, 2015) and 'hierarchical'. In particular, economics is dominated by a small number of top departments and journals to an extent not observed in other disciplines (Han, 2003).

Economists interviewed were distinctive for their unified account of their own discipline. Despite being spread across three countries and a range of research foci, economists tended to express similar criteria for evaluating methods, similar frustrations with the literature, and similar hopes for the impact of their work. This does not mean interviewees applied economic theory and methods uncritically, or without creativity. However, there can be little doubt that normal science within economics is inflexible compared to other disciplines in HIDR, and this rigidity was reflected in attitudes about quantitative methods. Economists interviewed placed very high value on quantitative methodology, and three (out of five interviewed) were critical of what they perceived as lower standards in other disciplines. Several interviewees from other disciplines described communicating with economists as very challenging (see Chapter 10), but, rather than simply noting this challenge, it may be instructive to ask: Within what intellectual habitat is the economic approach useful? What special functions do economists value in quantitative methods?

As was mentioned in Chapter 3, it is theorised that strongly classified disciplines tend to empower academics, and this did seem to be reflected in economists' ideas about what makes a study 'good'. Interviewees with economic training didn't tend to list the attributes of an 'economically sound' study in the way epidemiologists did. Rather, sound methodology begins not with features of the study, but with the *researcher*. Common threads were the importance of precision in language, displaying 'care' in the conduct of analyses, and caution in drawing conclusions from data:

> TC: What are the hallmarks of good empirical work?
>
> […] The first thing is to be clear about what you are doing […] In causal analysis, the hallmark there is to be very clear on what it is you are trying to identify. What is your outcome? What is your treatment? What is the

> variable which we think is changing because of the effect? Defining those two, the outcome and the treatment very specifically, so we know exactly what is the effect that we are trying to identify? Then being super clear on how you are going to identify this effect. An empirical strategy which says "I am going to use *this* variation in my treatment."
>
> Health Economist A (Economics PhD)

Here is the abstract, atomised and cumulative nature of economic knowledge. The default position is not a connected set of factors, jointly influencing outcomes in context-dependent ways. To study causal connections in health economics is to carefully isolate one outcome, one treatment, and exploit variation in that treatment to quantify its causal impact. The grand aim is to *decompose* observed variation in health into constituent parts, neatly attributed to risk factors, exposures, or treatments:

> TC: What are the characteristics of good descriptive work?
>
> I think, certainly over the last thirty or forty years we have made great strides in developing better measures, we understand much better how we can measure and decompose, sometimes we call them "sources" but I have to be careful because I'm using causal language there, **it is certainly decomposing the association between socioeconomic status [and health] in some form**. Economists prefer to keep income and education separate, and not as a construct which people call "socioeconomic position".
>
> Health Economist C (Economics PhD)

The combination of income and education under the common label of "socioeconomic position" is undesirable for this economist. It seems that, among the health economists I spoke with, the special function valued most highly was decomposition of observed variation in health.

The value placed on this kind of analysis was also evident in discussions of where 'care' in causal analysis ought to be directed. Being 'careful' seemed to relate overwhelmingly to one aspect of study design known within economics as *identification,* briefly touched on in the extract above. Despite similarities between health economics and biostatistics, the term identification has no formal meaning in the latter, but is a point of emphasis in the former. All health economists interviewed mentioned the "identification police":

> We call them the 'identification police', the [journal] reviewers who will scrutinize every sentence to say, *this* you can call a causal effect. […]
>
> [Later]
>
> In that causal work, we admire people who have clever ideas for what we call the 'identification strategy' […] finding some sort of exogenous variation, ideally in one of these social economic indicators (income, education, occupation), and if that [variation] is sufficiently exogenous you can look for its effect on health. Or vice versa, the effect of health on one of those. But, very often, they are difficult to find.
>
> Health Economist C (Economics PhD)

Causal analysis in health economics is therefore preconditioned on a successful search for exogenous variation, a 'shock' originating outside the study in no way feasibly connected to variables under study. Only if a study is *well-identified* can causal claims be permitted, and this was described as being annoyingly restrictive by all economists interviewed, and as giving rise to studies of strange or extreme phenomena, tangentially-connected to health. Emphasis on identification was reportedly reinforced by editors of top economics journals, understood to value the careful identification of causal effects over the importance or implication of research findings:

> Only if you go to the lower quality economics journals would you be able to publish without this very, very clear explanation of how you've identified the effect you're claiming is causal […] the problem in economics for now is that it becomes so preoccupied with causality that they publish anything which they think is probably causal, irrespective of how important the effect is.
>
> Health Economist C (Economics PhD)

The 'identification police' are perhaps a reflection of how important internal validity is in mainstream economics. If the necessary assumptions for a method's use are not met, an economist may prefer to abandon analysis altogether, regardless of the potential usefulness of the outcome. This can create tension across disciplinary lines:

> "What methods are appropriate? […] the standards are different. The Economist would say, to some questions "I cannot provide an answer", a public health researcher would say "That is stupid, because we need that answer!" (Laughing)
>
> Health Economist D (Economics PhD)

Is it better to have no evidence than evidence generated without the appropriate care and caution? From the economic standpoint, there is at least the possibility that it is better to have no evidence. One specific study of lottery winners (Cesarini et al*.,* 2016) was divisive. On one hand, the study was described as elegant, and prototypically well-identified (lottery winnings representing a random income shock):

> The best new piece of evidence [about health inequalities] that was added recently was a study in the QJE, the top journal in economics, by a Swedish American group who used the Lotteries of the last 30 years.
>
> Professor of Health Economics C (Economics PhD)

However, this same study was presented as an example of the negative impact the 'identification police' are having:

> It is very difficult to study the impact, during adult life, of income on health. Because, the way economists typically do this is to develop an experiment, or a quasi-experiment, it is very difficult to come up with a real, very strong income shock in a quasi-experiment. People have used something like the lottery, but that is not what we are thinking of, we are thinking of living in a poorer area in Glasgow versus a rich area in Glasgow, which is a city famous for its health inequalities. How can you develop an experiment, an empirical design that allows you to really measure the impact of that kind of income difference? I think it is fair to say that these studies just don't exist.
>
> Health Economist D (Economics PhD)

The income difference to which the above interviewee refers is *endogenous* (inside the model, not an external shock), and by definition is not well-identified. For this reason, for economists, studying this kind of income difference feels 'very difficult'. Representatives from other disciplines would almost certainly disagree, and propose ways to study these income differences, but those disciplines are not so rigidly organised around the decomposition of well-identified causal effects. If empirical work in epidemiology feels like treading on ice, research in economics feels like peering down a microscope: Only when the dials and lenses are lined up just-so can anything be reliably discerned, and such intense magnification has the effect of restricting the world to narrow slivers of things, rather than wholes. With scope to analyse only very particular kinds of variation in the drivers of health equity, the study of disadvantage across a lifetime may appear impossible via the economic paradigm.

So far in the thesis I have repeatedly reported that social scientists value depth and complexity in empirical work, and this contrasted against others' preference for simplification and abstraction, reflecting different (disciplinary) tolerances for what can be discarded and disregarded en route to analytical insight. Economists tended to have a higher tolerance for dispatching local detail to obtain an elegant answer, but this liking for simplicity and abstraction is not a matter of taste, or preference: for economists, quantification and abstraction are necessary steps toward answering the kinds of questions at the core of the discipline, toward accessing authoritative evidence. The economic epistemology (like all epistemologies) tackles a certain kind of problem, by generating a certain kind of knowledge, from certain kinds of data. This manifests as a strong preference for particular methods.

Therefore, while sociologists and economists are making vastly different methodological choices, my analysis suggests these choices are made for similar reasons: Researchers seek, value, and use methods which support access to the core concepts of their disciplines, the methods understood to reliably generate the kind of evidence considered authoritative. As ideas about authoritative evidence seem to vary across disciplinary communities, so too does methodological preference. Returning to the introduction of this chapter, the perception of economists within the wider HIDR network is an excellent example of a tool operating at a distance from the material for which it was developed (a methodological polar bear roaming the tropics). Economic methods were developed as problem-solving mechanisms *within economics*, a strongly classified field with little tolerance for diversity in method, but what seems rigorous to economists did not seem rigorous to other interviewees: The geographer who highlighted the 'inseparability of things' in Chapter 7 went on immediately to say that the apparent rigour of elegant econometric approaches was 'built on sand'. A social epidemiologist from a different country, and a different network cluster, expressed a similar sentiment:

> Economists, especially some types of economists, use mathematics all the time, it looks so sophisticated, so important, so elegant. But behind that, very often, nothing. It is a desert.
>
> Public Health researcher & Medical Doctor (Health Policy PhD)

The 'desert' is perhaps the space left by things discarded en route to abstract analytical products: Context, complexity and lived-experience. From one perspective, these are the necessary casualties of pursuing the 'real story', fully-decomposed causal effects. From another perspective, context, complexity and lived experience *are* the real story.

Perhaps unsurprisingly, economists interviewed reported feeling misunderstood:

> There might be preconceptions […] people might say yeah, ok, [I'm an] Economist, I'm just interested in money. I know the price of everything and the value of nothing, blah, blah.
>
> Health Economist A (Economics PhD)

The only groups to consistently report that other scientists made assumptions about them because of their training were social scientists (who reported that their work was perceived as "Unscientific" or "soft" in some settings) and economists (who reported being accused of being "cold" or "heartless"). These criticisms reflect the location of these disciplinary groups at epistemological extremes: discarding everything except well-identified, causal threads on one hand, and preserving and maintaining the 'messy' stuff (as one interviewee said) on the other. Here, again, disciplinary tension is more accurately described as a connected set of tensions: The special functions scientific tools are expected to perform within these traditions are markedly different, and this reflects the phenomena considered relevant, and the different kind of insights considered valuable.

> TC: Do people make assumptions about you because you have economic training?
>
> Oh, clearly […] [the] typical prejudice about economists, "they want markets", "they want competition", "they want rationality". We don't want anything, we just have our own ways of trying to disentangle causal evidence.
>
> Health Economist C (Economics PhD)

Difficulties predictably arise when economists enter scientific spaces within which 'disentangling causal evidence' is a lesser priority. Taken together, these analyses of qualitative and quantitative methods demonstrate that when scientific practice reflects 'special functions' at the extreme, researchers can appear to one another as glass-encased specimens from some other planet, wielding tools with no obvious purpose.

3.4.2 Epidemiology & Biostatistics in community settings

In the USA, five interviewees working within an epidemiological setting were co-located with and/or directly involved in community-based healthcare. As was discussed in Chapter 7, these scholars were described as 'interlopers' by some British scholars of the Health Inequalities research tradition. As interview data suggests assumptions are made about the way these scholars approach their work, and the things these scholars value, I take this opportunity to present their perspective and preferred approach to research.

These researchers seemed connected to their local communities in ways which British, Australian and Continental European interviewees were not, including regular contact with community members:

> I'm a nurse. So, that's probably the biggest… I still try to keep in touch with the population, so I practice what I preach.
>
> TC: How do you keep in touch with the population?
>
> Every Thursday afternoon I do a food pantry. It's in a very poor part of our town and I have my bag full of blood pressure cuffs. I know some of the

> disparities that are happening within our community regarding overuse of the emergency room, for fevers. So I hand out thermometers, and talk about how you can bring down a fever without going to the emergency room. When people come in sick, to get food, we talk about what would be the best plan.
>
> Nursing Researcher (Nursing PhD)

This connection to community was evident in the way interviewees viewed research. Three interviewees explicitly characterised their academic career as a service to community (no other interviewees explicitly framed their research in this way):

> Some of the greatest lessons I've learned as a scholar came from my community partners […]. I might understand aetiology, I might understand the statistics, yet they understand the implications of that and they know what they see, what's behind those numbers. […] I call them my most influential teachers […] [my work] has to involve and has to engage the broader community I seek to serve.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

Researchers with certain ideas or priorities may self-select into these environments, these environments might actively shape understandings of good research as involving the community, or both. In any case, these five researchers had quite particular ideas about what good quantitative research looks like, and some of these ideas stretched or cut against the norms of classical epidemiology.

They valued multi-level analyses:

> I love a good multi-level study. I love it when there's this recognition that the individual alone isn't the sole indicator of success or failure of an intervention, multi-level interventions specifically. […] does the bus route come near here? Is this walkable? Is there a pharmacy in the area? […] the culture and the system […] I like that kind of stuff. Helping to understanding what the conditions look like which can positively and favourably promote decisions which are conducive to health is important.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

And also expressed a preference for adaptive or dynamic study design:

> If you want to change individual behaviour you really need to be thinking in this more pragmatic way of doing things. The idea of randomising people to

> get something, especially after informing them, in my mind is borderline cruel and methodologically not all that sound. […] [We need] to meet the exigencies of life, and the changes in circumstance.
>
> Professor of Health Sciences (Epidemiology PhD)

Accommodating the 'exigencies of life' is not a usual consideration in the design of epidemiological intervention studies. This group of researchers (frequently in contact with the reality of disease within a single, materially deprived community) appeared to value methods which perform the special function of accommodating, or somehow recognising those lived realities.

Does this attention to context suggest a lesser role for disciplinary training? A comparison of these researchers with the social scientists conducting qualitative work suggests not. As was discussed in Chapter 7, the general aim within health promotion is to intervene (to 'change individual behaviour' as the interviewee above expressed). Similarly, the North American researchers embedded in community-care framed their research specifically as aiming to modify individual behaviours, and did not advocate for research methods which studied social structures or policy processes. Despite clear insight into the structural determinants of health in their communities, these researchers did not discuss 'structure', 'agency', or 'society' as objects for study. Rather, good research accommodates life-within-community via adaptive trial designs, and health disparities are combatted at food pantries with bags full of blood-pressure cuffs, one individual at a time. The goal of research in these setting seemed to be less focused on understanding the social structures which create or sustain inequality, but rather understanding how to change behaviour *within* that social context, by identifying the optimum configuration of interventions which support individuals to improve their own health. These researchers advocated for a mixture of qualitative and quantitative methods, which perform the special function of confirming the acceptability and suitability of interventions within community settings:

> When you draft up an intervention, you ask, what is the best way to deliver

> it? Is it in print, is it via phone, video, classroom, from the doctor, what works the best? Once we have marked things up, we show it and say, "Does this look ok? What colours do you prefer?" We really do everything.
>
> Cancer Researcher (Epidemiology PhD), Cluster 3

Therefore, qualitative methods perform a different function for these researchers, compared to the social scientists in Clusters 4 and 6 (discussed in section 8.2.1). The contrast between community-oriented researchers and social scientists interviewed was also evident in discussions regarding where the 'answer' to health inequality and disparity is understood to be located. Researchers embedded within communities tended to look to community for solutions:

> If the problems are in the community, so are the solutions. I firmly believe that, and embody that, and live that in my work.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

The researchers in Clusters 4, 6 and 7 who expressly valued research about society and policy did not locate the solution in the community, and some explicitly rejected this approach:

> I'm on the more structural side of the structure-agency debate […] the [research] focus right now is very much on the individual health behaviours, and then a bit of community, because, why not put poor people and poor communities under pressure to deal with their own health problems?
>
> Public Health researcher (Social Science PhD)

This brief analysis of one specialised research context adds some further complexity to the qualitative/quantitative divide. We see that the fervent pursuit of bias-free quantitative estimates takes a back-seat in some settings, and methods are required to perform an additional special function, accommodating and reflecting the needs of the community. Additionally, qualitative methods can be moulded to different purposes, and perform different functions across disciplines, and in different geographical contexts.

8.4.3 Social Epidemiology

Social epidemiologists tend occupy a position in the centre of the citation network (Figure 10), between the biomedical-dominated right hand side and the social-science dominated left hand side. This reflects the central aim of social epidemiology, to integrate ideas about health from the social and biomedical sciences. Social epidemiologists were the only epidemiologists to foreground theory as a hallmark of good empirical research, or to discuss qualitative approaches and/or mixing methods. Social epidemiologists also stressed the importance of placing quantitative results within social contexts:

> TC: What are the hallmarks of good empirical work?
>
> Difficult to say. I like ones that recognise that whatever the findings are, they need to be placed in context. They need to be contextualised […] I'm not looking for a general, universal theory of everything […] why does something work in a particular setting? And what are the factors there which influence it? That sort of thing.
>
> Public Health researcher & Medical Doctor (Medicine PhD)
>
> When I say quality I mean many things. Many things need to be taken into account, including the conceptualisation. […] sometimes we need a mixed methods approach, a systemic approach, [or] an historical approach.
>
> Public Health researcher (Health Policy PhD)

Despite this attention to context and tolerance for varied approaches, social epidemiologists shared the mainstream epidemiological concern about error and bias:

> [Good work includes] a real accounting for what the biases are that threaten the validity of A) the construct, B) any empirical measures ostensibly reflecting that construct, and C) the limitations of the inferences themselves based on the kinds of data that are available.
>
> Social Epidemiologist B (Epidemiology PhD)

As research methods tend to signify and symbolise particular intellectual domains, it is not surprising that augmenting the epidemiological epistemology with elements from the social sciences was described as personally rewarding, but professionally very challenging (in Chapter 7, one social epidemiologist highlighted the difficulty of 'getting understanding and acceptance from colleagues' when living in the two worlds of medicine and social science.)

Some social epidemiologists did not feel their approach is understood or valued within wider epidemiological circles:

> [In epidemiology], the problems and some of the limitations have partially, only partially, in the last few years have been developed. Like geographical epidemiology, or political epidemiology, other things. But, still, this is not the mainstream, this remains a minority view in this science.
>
> Public Health researcher (Social Science PhD)

And focus on the technicality of research design was frustrating for some social epidemiologists who felt this occurred at the expense of theory:

> In my view the good social epidemiologists have become marginalised and epidemiology has become more and more self-consumed […] [the] more technical issues have become more forefront, I think.
>
> Health Equity Researcher (Sociology PhD)

Therefore, social epidemiologists are aiming to strike a very challenging balance: demonstrating the hallmarks of good research from more than one epistemic culture. The role of theory within social epidemiology is central to understanding the kind of insight valued by members of this sub-specialty. In the next section I consider theory in detail, a scientific tool unexamined so far in the thesis.

8.4 Theory

My interview data highlight a lack of agreement across disciplinary groups regarding what scientific theory is, how it should be used, and what function it serves. However, this is not an open scientific debate or controversy, as no interviewee described active contestation about what theory is and/or what it does. Notions of theory seem to be located within the heart of disciplines, as a resource *for* the discipline, not to be advanced or extended toward others. Even economists (described as ‘colonial’ by some interviewees, in their perceived efforts to sweep across intellectual territory) described theory as something to be discussed amongst themselves:

> Epidemiologists […] I never can talk with them about my theoretical stuff, that is something I only do with other Economists
>
> Health Economist D (Economics PhD)

Apparent in the data was a spectrum of theory-related terminology which seemed to reflect an underlying continuum of ideas about both what theory is, and what it does. This continuum, depicted in Figure 16, traverses the abstract and the concrete, with a somewhat fuzzy centre:

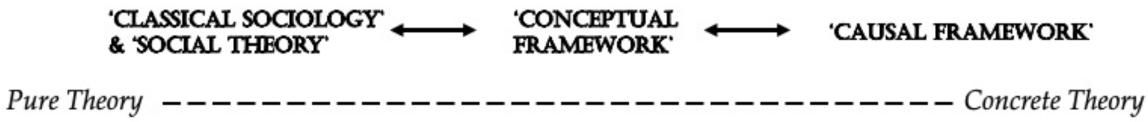

Figure 16, terms employed in discussion of theory in HIDR

Here is a recapitulation of findings from Chapter 7. Just as knowledge about society was frequently pitted against knowledge about disease, theory about society (social theory) appeared in opposition to theory about disease (causal theory). These terms map onto material covered in Chapter 7 in two further ways: First, within disciplines, 'theory' acts to reflect the objects of principle scholarly focus. In epidemiology, these are the links between exposures and outcomes (see Section 7.1.2). In the social sciences, this is social structures and phenomena (see Section 7.1.1). These are not an unexpected findings, but, as I elaborate below, comparing the use of theory in these disciplines demonstrates the way theory blossoms out of a discipline's motivating focus, rather than being a detachable epistemological accessory.

Second, the form theory takes seemed to correspond broadly with disciplinary preferences for pure and applied knowledge. At one end is 'pure' theory about society, denoted in interview data by references to "Social Theory" and theories "belonging" to Sociology. At the other end are concrete theories about the causes of disease, denoted by references to "medicine", "hypotheses" and "causal frameworks" as theory. In the centre, somewhat awkwardly, sits the “conceptual framework”. Discussion of this term and rhetorical function it appears to perform close out this chapter section.

8.4.1 Theory as Hypothesis & Causal Pathway

Interviewees trained in public health and epidemiology frequently employed the term theory as a synonym for hypothesis, sometimes as a comment on the discipline:

> In public health, theory is often a hypothesis, essentially. ‘Thing X works in this way on thing Y’.
>
> Population Health Researcher (Public Health PhD)

Just as qualitative methods tended to be evaluated in terms of what they could add to quantitative research, this technical view of theory-as-causal-pathway was presented in terms of its capacity to improve statistical analyses:

> You have a theory about what would be a confounder, what would be a mediator, and why. And you start your causal diagrams and build your statistical model on the basis of your theory. And let that theory guide what variables you actually put in your model.
>
> Epidemiologist B (Epidemiology PhD)
>
> You need to put structure on it [a statistical model], so you need to bring your theory to bear.
>
> Epidemiologist & Medical Doctor, (Medicine PhD)

The discipline of medicine was referred to as theory in a similar way, both by medical doctors and their collaborators:

> Biological plausibility comes from theory.
>
> Public Health researcher (Medicine PhD)
>
> People with medical training, they are much, they feel much stronger on the theory, this conceptual understanding of a why a certain [medical] condition could be varying.
>
> Health Economist A (Economics PhD)

The special function performed by this kind of theory is improved model development, and guidance for selecting variables in quantitative analysis. In addition to guiding what goes into statistical models, theory about the causal pathways driving disease prevalence was described as guiding the interpretation of what comes *out*:

> If I put a variable in my model and I see that these two [variables] are adjusting the main association I'm interested in, I could interpret it as either being due to confounding, or I could interpret it as being due to mediation. What it is depends on my theory. It is not the statistic that tell me this.
>
> Professor of Epidemiology and [Blank] (Epidemiology PhD)

These references to 'theory' do not refer to social theory as a social scientist would understand it (see below), but, to ideas about how variables represented in a statistical model are understood to relate to one another. These ideas appear to be personal*.* The interview extracts above make clear that statistical interpretation and variable selection depend on *your* theory, *your* causal framework, *your* conception of the factors which collectively and separately determine health and disease.

The application of this kind of theory was also described as being time-sensitive. Interviewees described a window within which causal theory can be usefully exercised, which closes once a statistical model is run:

> [I] believe more in getting theory before you start building your model. In social epidemiology […] you need to have the theory before you start building your regression model.
>
> Epidemiologist B (Epidemiology PhD)
>
> I see it in studies from epidemiology […] you notice that they haven't thought about any conceptual framework, the causal framework […] There is a lack of causal thinking before the analysis is made.
>
> Social Epidemiologist (Epidemiology PhD)

This time-sensitive application illustrates the perceived fragility of this kind of theory, relative to quantitative findings. In the epidemiological epistemology, once a model is run, 'causal theory' survives on the basis of statistical confirmation. However, things are not quite as simple as was just suggested. If causal theory ('causal thinking', as one interviewee just put it) informs both the input and interpretation of statistical models, there must be an epistemological mechanism by which causal theory can survive a disagreeable finding. This mechanism is outlined and discussed in Chapter 9.

8.4.2 Classical Sociology & Social Theory

The view of theory-as-causal-pathway was contrasted against the view and use of theory within the social sciences by several social scientists, and social epidemiologists:

> Usually epidemiologists don't have a theory. Quite often actually they have a hypothesis derived from coffee table discussions, or whatever. They have an idea, is there a positive relationship, or not?
>
> Health Equity Researcher (Sociology PhD)
>
> Theory is not "I've got an idea." It is much more complicated than that.
>
> Public Health Researcher (Social Science PhD)
>
> It [epidemiology] has theories, but it has mainly pathways and mechanisms and explanations. That is not theory.
>
> Sociologist (Sociology PhD)
>
> To me, the big difference between social science in public health and true discipline-specific social science is about the use of theory. […] If you are a true social scientist or a true sociologist, theory is something epistemological, kind of a meta-thing that comes before the question and frames the entire way of thinking.
>
> Population Health Researcher (Public Health PhD)

No interviewee stated definitively what theory is in the social sciences, consistent with the integrative collection code and relatively less-strong classification of these disciplines (compared with epidemiology) discussed in Chapter 7. But, use of the term "social theory" (rather than "causal theory") was consistent among social scientists and social epidemiologists.

> We're [Epidemiologists] not based in the social theory that I think would be

really helpful for some of these questions. […] I think it [epidemiology] is lacking in its basis of social theory.

Social Epidemiologist A (Epidemiology PhD)

One interviewee did outline the all-encompassing nature of social theory in terms more precise than a "meta-thing":

> The topic you choose, the way you choose it, the topic, the problem, the objective, the methods, the way you interact with the methods, the analysis, the data, everything is involved with a theoretical view or vision of what you are trying to understand.
>
> Public Health researcher (Health Policy PhD)

Clearly, this is not the same scientific tool as was described in the previous section. Social scientists described theory as acting in many more ways, and on many more parts of the research process than other disciplines. There is no time-limited window, no restriction of theory to causal pathways (to links between concrete variables) and no emphasis on statistical confirmation. Additionally, social theory is very rarely 'yours' in the way causal theory appeared to be. Quite the reverse, as social theories are frequently named for their originators, and names (like Bourdieu, Foucault and Latour) can act as shorthand for sets of theories[13].

The special functions of social theory were visible in interview data, despite their flexible and amorphous nature. One key function was to grant analytical access to the principle objects of scholarly interest in the social sciences: social structures, policy processes, and processes of social change. Interviewees in network clusters 4 and 6 (the most social-science-rich region of the network) described theory as having two more specific functions: directly supporting analytical insight, and transforming researchers' thinking:

> Critical [blank] theory […] that's the kind of thing you don't get elsewhere, where you can have real lightbulb moments and it flips your thinking about

[13] It is interesting to note that in epidemiology and biostatistics, with the exception of the Hill criteria for causation (after Bradford Hill), names typically act as shorthand for statistical tests, frequently used to test hypotheses (causal theories).

> things.
>
> Public Health Researcher (Health Services Research PhD)

> It is a spotlight on something, on some findings, that just adds this much *clearer* explanation of what's going on. […] when I read good theoretical work, I think it contributes a new way of thinking about something.
>
> Health Policy Researcher (Geography PhD)

Social theory appears to provide this support by rendering visible what was previously invisible:

> "Oh yes I didn't see that!" It was before your eyes, [but] you were not able to understand it.
>
> Public Health researcher (Social Science PhD)

However, theories were not viewed as equal in this regard, and, like all scientific tools, the application of theory in the literature was subject to critique. Social scientists described encountering work which uses theory to varying degrees of sophistication and success:

> You get a lot of work that engages with a particular theoretical approach in quite a shallow way. That is definitely the case in policy studies where people seem to gravitate towards [blank]'s model and reproduce it in a fairly unengaged way. Sometimes it's not clear that they've read the original work, and even if they did, it's a fairly simple way of thinking about things.
>
> Health Policy Researcher (Geography PhD)

Here again is the presentation of simplicity as a weakness within the social sciences (specifically, simplicity in *thinking*). Perhaps this is another special function of 'good' social theory, that it supports the researcher to preserve and manage the complexity of social phenomena, established in Chapter 7 as a key virtue within social science disciplines.

Just as epidemiologists varied in their preferred application of statistical methods, social scientists expressed diverse views about and preferred use of theory. Generally, sociologists tended to be dissatisfied with the application of theory in HIDR, desiring more depth, and rigour:

> What has been commonly used is Pierre Bourdieu, with his capital, that is

> one area. But it shouldn't just be applied and you have a half-page introduction, it should be thoroughly discussed and argued.
>
> Sociologist (Sociology PhD)

For similar reasons, some sociologists viewed HIDR as being weak in theory, and viewed this as a consequence of its disciplinary makeup:

> It is a really theory-weak field. Of course, one of the reasons is it has been dominated by medical researchers for too long, which is also why it is very statistically oriented.
>
> Health Equity Researcher (Sociology PhD)

Recalling Section 8.2.3, the focus on measurement within epidemiology is a central pillar of that discipline, unlikely to shift. But this focus was perceived by some social scientists as creating an atomised evidence-base:

> TC: Do you feel there is sufficient theoretical underpinning for work about health inequalities?
>
> No. Much of it in fact seems devoid of theoretical underpinnings, or of the need for them. "Here is something we can measure, great, let's do it!" Fine, but what is the connection to anything else?
>
> Health Policy Researcher (Social Science PhD)

Of course, it is only from the vantage of an integrative discipline that cumulative knowledge structures appear unsatisfactory. The special function 'connect findings to everything else' is not high on everyone's priority list. However, this view of theory as demonstrating the connection of empirical findings to broader ideas was a point on which even economists and sociologists appeared to agree. In *all* disciplines, in both qualitative and quantitative settings, theory was described as helping to discern the meaning and implication of empirical findings:

> Even a well-designed RCT cannot give you all the answers, you still need theory, to develop ideas about how things work.
>
> Health Economist C (Economics PhD)

In this way, theory appears to perform the special function of *safeguarding against superficiality and meaninglessness,* a 'special' function indeed. This may explain why members of scientific disciplines take such fierce pride in their theories, and experience criticism or dismissal so deeply. Latour & Woolgar (1986) described the crushing persistence of chaos within scientific inquiry, and noted that much of what researchers do appears to be aimed at carving out local “pockets” of order, within which certain ideas can be tested. Theory appeared to help interviewees to create order (in their thinking, and in the world) and this function cut across epistemic cultures. However, as my previous analysis has demonstrated, ‘depth’ takes different forms in different disciplines, and so it should not be surprising that the term ‘theory’ does have consistent meaning in varied disciplinary contexts. Rather, theory takes different forms, enters the epistemology at different times, with different fragilities, and for different analytical purposes.

8.4.3 The Rhetorical Import of “Frameworks”

Based on the preceding analysis, social epidemiologists, working at the intersection of the social sciences and epidemiology, might be expected to experience some difficulty in coherently combining the two approaches. However, for social epidemiologists the ‘conceptual framework’ appeared to act as an important rhetorical resource:

> [Some]thing that I’ve found is that some clinical epidemiologists are more open to a “framework” (laughing) and you can then use a “framework,” and test a “framework”.
>
> Epidemioligst C (Epidemiology PhD)

Several social epidemiologists described needing to temper their use and discussion of social theory in clinical (or epidemiological) settings. In these settings, social theory performs no recognised special function, because it does not provide access to a phenomena with which clinical researchers are familiar, and may not connect obviously with insights of value. However, this is got over via the rebranding of theory as ‘conceptual framework’.

The extract below, a response to my question about whether this interviewee (trained in anthropology) feels social epidemiology is sufficiently theoretical, reveals the way in which the conceptual framework functions as a sort of epistemological halfway-house between social theory and causal theory:

> Many social epidemiologists do have a framework, or a conceptual framework, at least in their head, of how social factors impact health outcomes. And the chain of causation which underlies that. […] Social scientists are more theory-driven than epidemiologists are. But I think most have a conceptual framework in their head.
>
> To what extent that there are deep underlying theories behind those conceptual frameworks? I don't know. I suppose it also depends on how detailed or deep a theory needs to be to be called a 'theory'. But, people definitely have conceptual frameworks.
>
> Public Health Researcher (Public Health PhD)

What interviewees meant by 'conceptual framework' seemed therefore to represent the maximum theoretical content tolerated by clinical epidemiologists, but perhaps the minimum viewed as acceptable by sociologists (a negotiation which makes sense in light of clinical epidemiology's relatively stronger classification, and the power wielded by some epidemiologists in their role as journal editors). This was the reason that some sociologists did not view social epidemiology as productive blending of sociology and epidemiology. Rather, social epidemiology was presented as a watering-down of sociology for presentation and publication in epidemiology journals, a kind of 'sociology-lite' :

> In the last 10 years I have been much more inclined again to see myself as a sociologist [rather than social epidemiologist], because there are inherently sociological issues in how people act, and behave, and interact. I have come to think that social epidemiology has less to offer, if you want to have more theoretical understanding of the processes at work.
>
> Health Equity Researcher (Sociology PhD)

The challenge of social epidemiology is now a recurring theme in this chapter, and within the thesis. In Chapter 5, social epidemiologists were located at the centre of the bibliometric network, linking diverse scholarly communities across citation-space. Chapter 7 demonstrated that knowledge about disease and knowledge about society take different forms, and are organised via different structures. In this chapter, the challenges of combining social and biomedical approaches became clearer, as methods are inextricably linked to the insights they support, and to phenomena on which they shed light. At risk of simplifying to an unhelpful degree, there is meaningful tension between a sociologist's desire to understand, the economists' desire to decompose, and an epidemiologist desire to measure and intervene, and this tension appears to touch all areas of scientific practice.

8.5 Conclusion

In this analysis, the connection between paradigmatic concepts and researchers' sense of what is worth doing was explored in detail. Disciplinary concepts and concerns may drive methodological preference, or researchers may focus on concepts to which they can confidently gain access, via methods with which they are familiar. In either case, methods are not simple tools to be pick up and put down. The decision to employ a method seems to have three precursors; being familiar with the method, familiar with the phenomena the method is suited to investigate, and belief that resulting insights will have value.

Similarly, preference for abstraction versus complexity appeared to be related to whether complexity is understood to hinder or constitute valuable analytical insight, with what researchers 'value' being at least weakly influenced by disciplinary training. Also different across disciplines (and connected to particular forms of knowledge) were views of theory, which, like research methods, perform the function of supporting researchers to access the kind of knowledge they value, and to carve out pockets of order in their thinking, and in the world.

Therefore, analysis suggests that whilst researchers make different methodological choices, these choices are made for similar reasons.

Researchers seek, value, and use methods which support access to the core concepts of their disciplines, and prefer methods understood to reliably generate the kind of evidence they consider authoritative.

The next chapter focuses on statistical methods, the concept of 'statistical significance' in different disciplines and the extent to which interviewees from different disciplines view two particular statistics as representing straightforward 'truth'. The empirical part of the thesis then concludes in the following chapter, with an investigation of how these tensions may complicate attempts to collaborate across disciplinary boundaries, and how interviewees navigate these difficulties.

Chapter 9. The Eye of the Beholder: Meaning and Statistical Inference in Quantitative Health Equity Research

> Scientists themselves constantly raise questions as to whether a particular statement “actually” relates to something “out there,” or whether it is a mere figment of the imagination, or an artefact of the procedures employed. It is therefore unrealistic to portray scientists busily occupying themselves with scientific activity, while leaving debates between realism and relativism to the philosophers.
>
> Latour & Woolgar (1986), Laboratory Life, p.152

9.1 Introduction

The results presented thus far connect with the existing, overlapping literatures of STS, SSK, Higher Education Studies, English for Academic Purposes and the Sociology of Professions. To various extents, interest in academic disciplines peaked in these areas between 1980 and 2000, and has subsided more recently. Statistical practice has, however, never been a fashionable focus within these fields[14].

Mackenzie (1981) provided a thorough account of the ways social experience shaped statistical practice in the UK between 1865 and 1930; for example, Francis Galton’s views on intelligence, heritability and eugenics demonstrably ‘conditioned the content’ of his statistical practice (Mackenzie, 1981:p.66). To date, very little empirical research has investigated what conditions the content of 21st century statistical practice, even as this practice becomes increasingly important within public health and medical research. To my knowledge, no empirical account of factors influencing individuals’ statistical practice in public health is available.[15] Several studies evaluate statistical knowledge among particular groups

[14] This chapter was presented at the July 2020 meeting of the Australian Statistical Association, Victorian branch, recording available at https://vimeo.com/ssavic/2020721-collyer

[15] Currently underway, but not part of this PhD, is a scoping review of literature surrounding statistical practice. The references included here are representative of the 47 studies included at the abstract screening stage.

(Ercan et al., 2013; Badenes-Ribera et al., 2016) or summarise what is reported in particular journals over time (Goldin et al., 1996; Armstrong & Henson, 2005). Many such papers begin with the aim of illuminating errors, misunderstandings or 'bad practice' (Bland & Altman, 2003), and the overwhelming majority focus on research published in psychology and medical journals, rather than population health.

Searching Scopus and Web of Science[16] for the phrase "sociology of statistics" returns just 9 results (Bardet, 2008; Camargo, 2009, 2015; Desrosières, 2007; Otero, 2018; Prévost, 2009 ; Sabri, 2013; Senra, 2008; Thévenot, 2011) Of these, none relate to health, but to a diverse set of topics including student evaluation, home care, policy evaluation, fascist Italy, French Philosophy, and a review of the historical study of statistics. Camargo (2009) identifies the sociology of statistics as a potential new field of study, describes the study of statistics at a macro level, as a political technology, and at the meso level within 'statistical institutions'. While this focus will doubtlessly yield interesting insights, the framing of statistics as belonging to institutions or groups leaves little room for the human analyst, and risks treating individuals within institutions as homogeneous. Additionally, the scientific research context has unique organisational characteristics (Knorr-Cetina, 1999) suggesting that insights from non-academic institutions may not transfer to research. This paucity of work stands in contrast to the currently consolidating (though loosely connected) subfield known as the sociology of quantification (Berman & Hirschman, 2018). Four thematic areas have been identified, extracted below from Berman and Hirschman (2018, p.258)

- The technopolitical decision-making that guides methodological choices […] showing how social, technical, and political factors interact to make stable numbers.
- Which kinds of numbers matter and when?
- How do we govern quantification?

[16] Searched 03/10/2020

- How should scholars study quantification?

The focus of the work reviewed by Berman and Hirschman is very much on statistics in society, politics, big-data and the quantified self. My focus on statistical practice in health-related research settings and the role of that practice on the construction of scientific knowledge is therefore not an easy fit, but does perhaps contribute to the first two themes, as limited to research contexts.

In Chapter 6, the network visualisation demonstrated the way geographic, historical, institutional and disciplinary factors intersect to jointly influence citation patterns in HIDR. It is possible that a similar set of forces shapes statistical practice. In this chapter I take a micro view of statistical practice, focused on the way individuals understand and interpret statistics in research settings, and ask: What account do researchers studying health equity provide of their own approach to statistical work? What variation is present regarding the kind of information understood to be contained in statistical output? Returning to the parlance of Chapter 8, what special function do researchers understand statistical analyses perform, and does this vary by discipline? A whole thesis could have been completed (and may yet be) on the topic of how researchers within public health understand the statistical methods they use. In this chapter I answer these two questions as best I can from my data, and outline directions for future research.

To narrow the scope of enquiry I focus on two specific statistical tasks: The interpretation of regression coefficients and the interpretation of p-values. These were selected for the near ubiquity of regression methods in quantitative health-related research (Hidalgo & Goodman, 2012), and the heated, long-running debate surrounding p-values and their appropriate use (Wasserstein & Lazar, 2016; Matthews, Wasserstein, & Spiegelhalter, 2017). In addition, my experience in biostatistics leads me to suspect that there is variation in the way these statistics are understood.

While there is not a mature sociology of statistics on which to draw, three concepts from the SSK and STS literatures help to more precisely investigate statistical practice; These are Duhem's Paradox (Duhem, 1954) , the Agonistic Field (Latour & Woolgar, 1986) , and Mechanical Objectivity (Daston & Gallison, 1992). Before presenting interview data I introduce these concepts, below.

9.2 Duhem's Paradox & the Agonistic Field

Variation in interpretation of statistical results may arise due to what is known as Duhem's (1954) paradox. The 'paradox' is that no scientific result can be unambiguously interpreted without reference to an existing set of theoretical propositions. For example, an experiment designed to detect gravity may fail to do so, a finding with two interpretations: either the experiment was incorrectly conducted, or gravity is absent. By referring only to the content of the experiment researchers cannot know (definitively) which interpretation is correct. Within HIDR, faced with unexpected results, researchers may find themselves unable to judge whether results support a revised understanding, or suggest the study was flawed. If researchers rely on existing theoretical propositions to interpret ambiguous results, it seems plausible that these are acquired during disciplinary training, and may vary across disciplinary groups.

In quantitative research about health, the interpretation of a beta coefficient (a component of a regression model) is an arena within which Duhem's paradox is encountered. When a researcher executes their syntax and regression results appear on-screen, they see something like Figure 17 below (coefficients are bolded):

	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
acs_k3	**-2.681508**	1.393991	-1.92	0.055	-5.424424	.0614073
meals	**-3.702419**	.1540256	-24.04	0.000	-4.005491	-3.399348
full	**.1086104**	.090719	1.20	0.232	-.0698947	.2871154
_cons	**906.7392**	28.26505	32.08	0.000	851.1228	962.3555

Figure 17 – A generic regression output with coefficients bolded

The bolded coefficients represent how some numeric outcome (Y) tends to change over different levels of an explanatory variable (X). It is on the basis of a table like the above that researchers argue, for example, that a treatment works, that inequality is present, or that a particular population is at increased risk of disease. But, in order to be interpreted, this numeric output must be parsed, and translated into statements about health. One interviewee described the appearance of the coefficients on-screen as 'beautiful':

> It's fun! The numbers come up […] All of a sudden, it's beautiful.
>
> TC: When the coefficients appear? What is it that's so wonderful for you, about that moment?
>
> Well, isn't that wonderful for everyone?? It's exciting! You get the answers. It's the end. Crossing the finish line. It's the cake coming out of the oven.
>
> Health Equity Researcher (Nursing PhD)

However, the analysis which follows suggests that the status of a regression coefficient as an 'answer' is a point of contestation, and that appearance of results on-screen is not the finish line. Frequently, it is the beginning of what Latour and Woolgar term an agonistic process (Latour & Woolgar, 1986), the shepherding of a new statement through an agonistic field:

> "Members of the laboratory are unable to tell whether statements are true or false, objective or subjective, highly likely or quite probable. While the agonistic process is raging, modalities are constantly added, dropped, inverted and modified." (p.150)

Generally, few statements survive the agonistic process. In Latour and Woolgar's own observations, negotiations among researchers as to what counts as proof, or what might be undermine a particular experiment's integrity, were disorderly, and frequently circular. Key stages in the journey from speculation to fact were characterised by the addition and subtraction of modalities, which served to tie new scientific claims to the local circumstances of their observation, limiting their generalizability: "it is the deletion of modalities which facilitates the promotion of speculative claim to fact-status" (*ibid,* p.69)

In the study of health, something like an agonistic process would be expected to be detectible surrounding regression coefficients, as they represent numeric proto-statements regarding relationships between exposures and disease risk.

Latour and Woolgar characterise facts as the set of statements which survive the agonistic process (and are therefore modality-free). Such statements are understood as being true by all ‘concerned parties’ (the members of the relevant specialty). This aperspectival objectivity (Daston & Galison, 1992) merits specific consideration in a discussion of statistical methods, as, perhaps more than any scientific domain, statistics have the air of being simply true, and of being beyond the influence of social forces (MacKenzie, 1981).

9.2.2 Mechanical Objectivity

As Victorian scientists sought increasingly to move away from reliance on the human observer, mechanical, automated images replaced artistic renderings as the preferred representation of scientific objects. Daston and Galison (1992) frame the search for objective renderings of nature during the 17th and 18th centuries as “a moral, as much as a technical quest” (p.117). Where human investigators proved lazy, inconsistent and unreliable, mechanical instruments entered the laboratory, and worked there, “patient, indefatigable, ever alert, probing beyond the limits of the human senses”(p.120). Mechanical objectivity emerged as a scientific ideal, and human judgement was re-framed as a source of scientific error, to be corrected, rather than a skill to be valued. Automated imaging technologies such as photography and the x-ray represented

> “attempts - never wholly successful - to extirpate human intervention between object and representation. Interpretation, selectivity, artistry, and judgement itself all came to appear as subjective temptations requiring mechanical or procedural safeguards” (p.98)

The search for objective imagery in science is therefore “inextricably tied to a relentless search to replace individual volition and discretion in depiction by the invariable routines of mechanical reproduction.” (p.98) But, the X-ray wasn’t a perfect truth-teller. Movement of the equipment or subject could result in certain contours disappearing (or appearing) in X-ray images. Thus, even mechanically generated representations required expertise in interpretation and “the acquisition of the ‘seeing eye’” (p.109).

Despite these weaknesses, mechanistic imaging devices retained an “aura of superhuman power”, a privileged epistemic status. The automation of image generation “excludes the scientists’ will from the field of discourse” (p.117) along with scientists’ fallibility and human weaknesses.

A similar transition seems have occurred in statistical inference (see Box 1) during the 20th century, as the statistical procedure known as Null Hypothesis Significance Testing (NHST) has come to dominate the quantitative analyses of health-related data. NHST is a mechanistic procedure, designed to remove expert judgement from the task of statistical inference (Gigerenzer, 2018). One of the biggest debates in all science currently surrounds the continued use of the hypothesis testing framework (Amrhein et al., 2019), with attempts to remove and replace it generally being regarded as unsuccessful (Matthews et al., 2017). It may be that mechanised, automated statistical inference procedures possesses the same ‘aura’ of mechanised images, and that the same forces which rendered the latter appealing are at work maintaining the dominance of the former, perhaps to varying extents in different disciplines.

In the following analysis I ask whether interviewees appear to be encountering Duhem’s paradox in their interpretation of statistical results, whether such results are reported as being shepherded through something like an agonistic field, and whether mechanised inferential procedures are received as the straightforward ‘words of nature’ (Daston & Galison, 1992), or whether they are interpreted through the critical lens of subject-matter expertise.

9.3 Interview Data

To support this analysis, I separated interviewees into four groups on the basis of their account of their own statistical practice:

- Researchers who mostly do their own quantitative analysis (n=18)
- Quantitative researchers who mostly delegate statistical analysis to others (n=19)

- Statisticians who conduct analyses for others (n=3)
- Qualitative researchers who do not conduct (or delegate) quantitative analyses (n=2)

These groups developed as I coded interview transcripts, and interviewees were allocated on the basis of data arising at all parts of the interview, most commonly in discussion of research methods, and statistics. I did not have sufficient data to classify one interviewee, and so they are missing from the summary above.

A general discussion of interviewees' sense of their own statistical literacy, and the forces in academia which appear to promote or hinder the development of statistical capabilities is not within the scope of this chapter, but is included in Appendix I. To summarise that material, interviewees had varying confidence in their statistical skills, which in many cases seemed inconsistent with my impression of their experience, and expertise. The status of 'statistical expert' emerged as being relative, a feature of expertise established by other STS scholars (Grundman, 2017; Jasanoff, 2003). For some interviewees (and, it was reported, within their teams), statistical proficiency seems to be surrounded by intense emotions (e.g., fear) and low statistical literacy was repeatedly connected to feelings of illegitimacy, inferiority and inadequacy. Despite this variation, the value and importance of statistical literacy was acknowledged by almost all interviewees, but most especially interviewees in the US working in cancer-related research, who presented statistical capability as being key for maximising research output, obtaining grant funding, and maximising individual employability. Despite being a valued skill, several senior academics across disciplines and geographic settings reported declining statistical literacy across their careers. Eight interviewees specifically mentioned that while early in their careers they conducted analyses themselves, they no longer do so. However, no interviewee in Cluster 2 (where negative knowledge was discussed as being highly valued) described themselves as being 'dependent' on statisticians in a similar way. Doing one's own statistical analysis was presented in tension with 'efficient' research output by interviewees in the UK, Australia, and the US. Despite a widespread perception that quantitative skills are highly valuable (even essential) in research settings, for some interviewees these are not as valuable as the efficient generation of grant applications and peer-reviewed publications.

Box 1: Statistical Inference

In applied research settings such as epidemiology and public health, probability theory is applied to study data via a process known as statistical inference. Inferential statistics support researchers to make claims (or, 'draw inference') about entire populations based on statistics calculated from a sample. The basis of inferential statistics in probability necessitates uncertain conclusions. Where in mathematics answers can be concrete and definite, in statistics answers are almost always couched in terms of chance (percentages, odds). In the face of this uncertainty, a process known as hypothesis testing is the dominant quantitative framework for generating inductive insight from sample data. Very generally, patterns observed in study data are compared with what might be expected, assuming a particular state of affairs known as the 'null hypothesis'. Where the observed data differs markedly from what would be predicted under the null hypothesis, that hypothesis is rejected in favour of some other explanation. In theory, under the null hypothesis significance testing framework, the researcher draws a bright line between results which have meaning (and merit the rejection of the null hypothesis), and results which do not.

9.3.1 Once more, with meaning

As was briefly mentioned in Chapter 7, interviewees presented the central conundrum of statistical inference as determining whether or not observed results have meaning. Also noted was the way interviewees trained in epidemiology and health economics placed high value on empirical strategies felt to safeguard against bias ('confounding' in epidemiology, 'poor identification' in economics), positioned as distorting results, or rendering them meaningless.

Some interviewees who lacked confidence with statistics indicated that this extended to their ability to determine which results have meaning, compared with statistical experts (whatever 'expert' meant within their context). Experts were perceived not only to have more, or broader understanding, but a qualitatively different understanding. It was in discussion of regression methods where five interviewees discussed the substance of this difference:

> The people who do stats that I really respect the most […] they really, *really* understand what regression is and what it's doing. I think they understand it from a qualitative sense […] there are a lot of people who can run unbelievably wizzy models but don't really have a scooby about what it actually means. They understand what the results are telling them but they don't understand, in a qualitative sense, what the model is doing.
>
> Public Health researcher (Geography PhD)

Understanding what a method 'is' and what it 'is doing' may not seem like expert-level knowledge. But, in my biostatistical consulting experience, few researchers can explain in detail how regression models work. Where interest in the modelling process is apparently limited, interest in results is widespread, and is typically focused on just one part of the regression equation, the 'effect size' or beta coefficient. Strictly speaking, removing a coefficient from its parent equation is not appropriate because the coefficient is only one part of the equation, and can only be accurately interpreted in context. Only interviewees in Cluster 2 noted this point (beta coefficient = 'odds ratio'):

> In medicine and epidemiology, often odds ratios will be presented as the way of showing an effect, which is not done so much in economics […] [also] there is the lack of appreciation that **an odds ratio really can only be interpreted in the context of the particular model.** Any odds ratio will be dependent upon the other variables you have in your model. So comparing odds ratios across studies, **that doesn't make much sense.**
>
> Health Economist A (Economics PhD)

In the discussion section of epidemiological and public health papers, the practice of contrasting odds ratios between studies is quite common, despite the technical issue outlined above. Whatever is gained by the comparison must surmount these technical objections, echoing the finding from Chapter 7 that the purposive slant of the epidemiological epistemology is a powerful driver of scientific practice: If useful knowledge can be gleaned, technical details may take a back seat. Recall that in chapter Chapter 7 some economists expressed the reverse position, and emphasised the cardinal importance of demonstrating that technical assumptions have been satisfied. Additionally, this economist's refusal to compare results across studies might hint at the origin and basis of the atomistic conceptualisation of health-related research common among biostatisticians and economists in Chapter 7. If results cannot be compared between studies, then health is, as some economists and statisticians in Chapter 7 described, a set of (disconnected) empirically-derived statements.

Multiple interviewees with economic training expressed the perception that epidemiological, medical and public-health publications often lack sufficient detail for a reader to independently assess (or even identify) the model which has been fit:

> It is hard to unpack, especially in public health and medical journals, what exactly has been controlled for, how they controlled for it, and what implications that might have.
>
> TC: Is that because only the final model is presented, and you have no idea what went on?
>
> Even when the final model is presented, only the coefficients of interest are presented, sometimes, you haven't got all of the other variables.
>
> Health Economist E (Economics PhD)

It is notable that for economists and some epidemiologists, this statistical detail appeared essential to the evaluation of claims made within a publication. Here we catch another glimpse of the role negative knowledge plays in these epistemologies. For quantitative studies of health equity, statistical analysis is one part of the research process where negative knowledge (knowledge about getting knowledge) and positive knowledge (knowledge about health) are in direct contact, and might mutually influence each other. However, disciplinary training appeared to shape the balance researchers wished to strike in this regard.

The only interviewees to comment on specific technical limitations of regression methods were economists or epidemiologists from network Cluster 2, or were biostatisticians, reflecting the importance of knowledge about the challenges of knowing, or limits to what can be known, in these disciplines and network cluster[17]. For some other interviewees, statistical detail was framed as a hindrance, obstructing the key message of a paper, obstructing the positive knowledge. For example, the following interviewee explained that sparse reporting is necessary, because the full regression model is incomprehensible to some readers, and extracting the central finding allows work to be more widely disseminated:

> I would say […] [to my statistician] "How the hell do we convey all of that?" My role is to turn it into something you can publish in a social science journal, rather than [*gushing sound*]. Nobody wants equations.
>
> Public Health researcher (Social Science PhD)

Despite this interviewee's own view, clearly, some researchers do want equations. These researchers tended to be very confident in their statistical literacy, and/or to be located in network Cluster 2. Here is evidence of the Clusters detected in Chapter 6 representing epistemic silos, as something indispensable to one researcher is presented by another as being a communicative hindrance.

9.3.2 The epistemological status of the beta coefficient

Given that the results of an entire analysis might boil down to a single number in epidemiology and public health, the meaning and interpretation of beta coefficients emerges as a key point of potential divergence in statistical practice. Even to the most accomplished and experienced analyst, the coefficient cannot explain itself. Scientific objects are not accompanied by a 'halo' conveying their meaning (Barnes, 1982); rather, their interpretation is something scientists learn, in a particular scientific context, within a

[17] In noting this trend, I do not claim that the limits of knowing are unimportant in the social sciences. However, when asked what sets good work apart from poor work, epidemiologists, economists and biostatisticians overwhelmingly referred to aspects of negative knowledge.

particular scientific culture (Jasanoff, 2003).

Acknowledging diversity in interpretation does not imply that all interpretations are 'true', as there are mathematically correct and incorrect interpretations, which either are or are not consistent with the form of the model within which the coefficient sits. But, not all interpretation depends on mathematical definitions. Two researchers who agree on the formal definition of the beta coefficient may disagree regarding what has been learned about health at the end of a given project, once the coefficient is known. In the next section I explore how interviewees understand these coefficients; what sort of information they contain, and whether researchers view coefficients as reflecting an entity which exists, or are a part of a symbolic mathematical sketch.

9.3.3 Regression as Knowledge Discovery and/or Construction

To try and understand what researchers believe a set of regression coefficients can convey about health, I asked interviewees familiar with regression methods (n=28) to discuss their conceptual approach to the application of these methods. Two pilot interviewees complained my question was 'abstract' and extremely difficult to answer. In subsequent interviews I invited researchers to position themselves along a spectrum: between viewing regression modelling as a process of knowledge discovery, knowledge construction, or something in between. Interviewees might consider the coefficient as something which is discovered by eliminating all bias and revealing the 'truth', or as something constructed by a researcher, a best effort, subject to major caveats. Nine interviewees articulated a clear position, three opted not to answer, and the remaining 16 responded with a general discussion about how they feel regression models correspond to 'reality' and 'truth'.

An unexpected question

Even with this specific line of questioning, several interviewees struggled to arrive at an answer. Four interviewees from a diverse set of disciplinary backgrounds and network clusters began by noting that they had never thought about this issue before:

Interesting! That is interesting. [Long pause]. I don't know. I don't know. […] I definitely understand both sides but I'm not sure I've ever thought about it in that way.	Health Education & Behaviour researcher (Health Promotion PhD)
Hmmm. I'm not sure I have a preconceived response to that question.	Health Equity Researcher (Sociology PhD)
[Long pause]. I haven't been asked that question before.	Health Economst B (Economics PhD)
[Pause]. I haven't thought about that. […] It's something to think about.	Social Epidemiologist A (Epidemiology PhD)

That researchers from such varied disciplinary backgrounds (and in such varied geographic locations) reported never having deeply considered the epistemic status of a statistic central to quantitative work about health is itself interesting. Although only four interviewees were open about never having considered the question, it was clear that the majority of interviewees did not have a prepared response. This is consistent with Kuhn's description of the paradigm as forming the unremarkable backdrop to routine scientific work, with Latour and Woolgar's (1986) description of inscription devices as representing black boxes opened only in the event of suspected malfunction, and also with Rudwick's (1985) conclusion, based on study of 19th century geologists, that the theory-laden character of empirical scientific work is only rarely contemplated by working scientists.

Three interviewees who commissioned analyses from statisticians but did not perform analyses did not directly answer the question, and seemed to view the issue as being outside their scholarly domain:

> Well, I've never really thought about… [trails off]
>
> TC: [laughing] Yes, it is a slightly weird question.
>
> Yeah. The regression. [Pause]. I just let my statisticians do all of the analyses. It keeps me honest. So, I don't have a view.
>
> Cancer Researcher (Epidemiology PhD)

Such responses suggest that some researchers who delegate statistical analyses view this as inclusive of the conceptual approach to quantitative work. For at least some researchers, the epistemological status of the analysis appears to be viewed as belonging to the analysis and not to the scientific project of which the analysis forms a part. Future studies might investigate this further.

Other responses to this question are handled in the next two sections, dealing with those who positioned regression as a process of discovery, or a process of construction.

9.3.3.1 Regression Methods as Discovery

Interview data contained contrasting perspectives regarding whether regression models can facilitate 'discovery'. Although no interviewee mentioned Duhem by name, his paradox seems ever-present in the interpretation of regression coefficients. Interviewees leaning toward the idea that models do facilitate discovery tended to reminisce about a particular result which surprised them:

> With our stuff on [blank] and inequalities, the model was the discovery. We ran a model and were not expecting to see a smaller inequality among populations that had greater access to [blank]. I wasn't expecting that, […] it was a bit like an archeological process, sweeping away the soil and suddenly, there's excalibur.
>
> Public Health Researcher (Geography PhD)

For this researcher, the beta coefficient suggested a revised interpretation of a commonly understood relationship, and this revised understanding was accepted as corresponding with the real world, rather than as reflecting a quirk in the study's design. But, the interpretation of unexpected results was not so clear for all interviewees:

> I am always a bit cautious. If you get something that looks weird, check it and see, and maybe test it using a split sample, or another dataset, or something. It should make sense. But I'm not ruling out the possibility that you find something that is completely left field. […] They may or may not be [correct], but at this stage you don't know what they are.
>
> Public Health Researcher (Medicine PhD)

This interviewee has articulated Duhem's paradox quite neatly, emphasising their inability to know what strange results mean at the time they arise. Faced with this uncertainty, knowledge from outside the model (from other datasets) is necessary to inform interpretation. Consistent across interviewees was the sense that such confirmatory references do not *provide* an interpretation, they inform the researchers' interpretation. From the above extract, it is "you" who doesn't know, and "you" who needs to evaluate the merits of the result and come to a decision. It is the researcher who decides whether a result has meaning (and represents 'excalibur', new knowledge) or is an artefact, to be dismissed or re-estimated. For the above interviewee, despite its mechanistic origin, a regression model does not speak the words of nature (Daston & Galison, 1992), the coefficient is a somewhat garbled statement to be treated with caution, to be checked and cross-checked against other sources.

But not all coefficients are treated with equal scepticism. The interpretation of coefficients seemed to depend on whether they agreed with researchers' prior expectations. Comments like the above, that coefficients "should make sense" were widespread, seeming to suggest that results fitting with researchers' existing understandings are somehow different to results which do not:

> My radar would be up more if it [the estimated effect] is an unexpected direction, or it was significant when I thought it wasn't going to be. You know, so I guess once that happened, I don't just take it at face value that "Yep, that's it". But I don't tend to do that for results that are sort of more expected.
>
> Health Behaviour Researcher (Health Behaviour PhD)

If the interpretation of the coefficient varies depending on whether it makes sense to the researcher, regression coefficients are not mechanically objective, but require a ‘seeing eye’ and expert judgement (Daston & Galison, 1992). The interviewee quoted above expressed this as follows:

> I think you have to take those [unexpected] results, but view them from a lens of common-sense

One strategy for navigating Duhem’s paradox is therefore the application of ‘common-sense’. As common-sense might rely upon things the researcher already knows, it seems that the paradox is embraced via this approach, rather than evaded. In the biostatistics literature, common-sense has been presented in tension with the Null Hypothesis Significance Testing framework for over 20 years. For example, Lane (1999) lamented common-sense as having been ‘sacrificed on the altar of statistics’ in medical research, meaning that expert, clinical judgement is systematically devalued. Later in the chapter, the specifics of what researchers mean by common-sense will be investigated more closely.

On the question of whether regression results represent discoveries, some interviewees, especially epidemiologists and economists, went around in circles. These interviewees seemed reluctant to rule out the potential for discovery, as memories of previous analyses or the certainty of seminal findings seemed to draw interviewees toward the conclusion that coefficients do represent ‘real’ relationships. But, as was discussed in Chapter 7, the persistent, nagging awareness of bias and measurement error seemed to prevent interviewees with epidemiological and economic backgrounds from settling on this as a definite answer. Three illustrative extracts are provided below:

> TC: [Do models provide discoveries, or represent a construction?]
>
> I am more on the ‘trying to find what’s real’ side […][But] it’s hard [for the coefficient] to ever be truth when we have so many caveats with measurement, and whatever. I hope I’m at least getting closer to the truth, as I’m building. But then, […] I guess I am ‘building’ […] Those estimates inform interventions, so let’s hope there is some truth to it. […] I’m hedging, I’m in between. Because, of course, it’s not perfectly true.

> Social Epidemiologist A (Epidemiology PhD)

For this interviewee, technical caveats get in the way of regression results representing simple truths. This interviewee finds themselves ‘in between’ awareness of the limitations of the model, and the sense that, over time and in the long-run, these methods do provide reliable representations.

> We know from regression, **if you’ve left something out of your model, you are not going to get perfect identification** of what’s in the model. So, yeah, I suppose I don’t generally view… [pause] Or do I? […] I’ve definitely changed my view about the world [following a regression analysis], my priors have changed. So **in that way I would be in the first camp** [model is discovery], **but, in general, I am in the second camp** [model is construction].
>
> Health Economist B (Economics PhD)

For this interviewee, technical limitations mean that results aren’t perfect, but, sometimes, regression coefficients do alter a researcher’s understanding of the world. This economist’s simultaneous identification with both ‘camps’ is yet another manifestation of Duhem’s paradox. Notice that ‘generally’, models do not facilitate discovery. Only in specific instances where the researcher’s prior understanding is updated (perhaps due to the absence of negative modalities) are regression models understood to have this capacity.

Seminal findings were frequently held up as evidence that regression models detect facts:

> Smoking does cause lung cancer. You’d really have to be philosophically nimble to say that that is somehow not a generalisable fact. You would bloody well hope that your case control study, or cohort study picks it up, and your model is therefore measuring something that is true. But it will have error, due to statistical imprecision, as well as the three sources of error: confounding, selection bias and measurement error.
>
> Professor of Epidemiology (Medical Doctor)

For this interviewee, Duhem’s paradox is seemingly evaded via acknowledgement that regression models access true relationships, but reflect them imperfectly. References to smoking and lung cancer were pervasive throughout discussions of regression methods, but interviewees employing this example seemed to ignore the heated debate which surrounded the original claim, and the range of perspectives and methods which contributed to resolution of the debate. In the above extract, it is known that smoking causes lung cancer before the model is run. It does seem straightforward that a model is ‘measuring something that is true’ when that truth is established in advance. Similarly, Latour & Woolgar (1986, p.153) noted in their study of molecular biologists that “the distinction between reality and local circumstances exists only after the statement has stabilised as a fact”. The splitting of truth and statement-about-truth can occur only after a scientific controversy settles, after a statement like “smoking causes lung cancer” stabilises within the agonistic field. Thus, Duhem’s paradox persists, as unless ‘truth’ is established at the outset, the presence of bias cannot be assessed by the researcher.

These data seem to suggest an agonistic process occurs around the interpretation of regression coefficients. Regressions produce ‘generalisable facts’ only in specific, particular instances, where bias, measurement error or other problematic features (modalities) are demonstrably absent.

One interviewee trained in psychology had a contrasting perspective, and argued forcefully that the special function of regression methods is to confirm pre-existing understandings, and that regression cannot discover (if ‘discovery’ means a finding not anticipated in a pre-specified analysis plan):

> You are testing the model. I mean, ((exasperated)) you don’t discover stuff, you verify it. When I’m doing quantitative [work] I am really into hypothesis testing. […] I have never found anything from statistical analysis that I didn’t expect to see […] the model doesn’t work that way. You’re not supposed to look at the correlation matrix and say “I have found a significant relationship I didn’t expect”, that’s not the way the model and the theory is supposed to work. I discover new stuff when I do qualitative stuff […] I don’t think you do [statistical] analyses to discover stuff, I think you verify and confirm.
>
> Health Policy Researcher (Psychology PhD)

This interviewee is adhering very tightly to the technical stipulations of rule-based null hypothesis significance testing, which mandate either that the researchers’ primary hypothesis is confirmed, or the null-hypothesis is not rejected and the analysis provides a 'null' finding (Pernet, 2016). Under this view, strictly speaking, when performing hypothesis tests, surprising or unexpected results should be discarded, and investigated in a dedicated study. However, in practice, where time and money have been spent on study design and data collection, and where researchers demonstrate their value by generating high-impact published outputs, surprising findings are routinely published. This practice has been positioned as problematic when researchers present unexpected findings as though the study was originally designed to detect them (Rubin, 2017; Kerr, 1998) If authors are deliberately opaque regarding the status of an analysis as either *post-hoc* or *a priori* then this is clearly questionable. However, the general validity of post-hoc analyses is debated in medical and biological science (Head et al., 2015) and there is no widely-accepted, formal process for reporting post-hoc analyses.

The above interviewee clearly views regression analyses as confirmatory, suggesting that regression coefficients are chiefly a mechanism for verifying what a researcher already suspects. In this way, despite taking a more extreme position on the limits of statistical inference, this researcher's view is not inconsistent with the other interviewees who implied, in various ways, that the interpretation of the coefficient depends on the researcher's existing understanding of the relationship under consideration.

9.3.3.2 Regression methods as knowledge construction

Several interviewees outlined a position indicating that regression models are a picture or sketch of reality, rather than representing reality directly. However, the reasons interviewees put forward as to why regression methods fail to capture the 'truth' varied quite neatly across two disciplinary groups. Social scientists tended to be reflexive and emphasise the role of their own values and priorities:

> It is a representation that, you know, as a researcher, you are constructing. Your own principles and kind of epistemological position, of course, are vital for how you've gone about that process. […] it is not like there is some sort of definitive truth out there, it is about thinking about defining a question and your own values, and the decision making that you implement

> throughout the research process.
>
> Health Geographer (Geography PhD)

Interviewees with epidemiology and economic backgrounds tended to locate the lack of correspondence with reality within the technical limits of the method, rather than the role of the researcher:

> It's an imperfect representation of reality, because there is always confounding. Also the variables and instruments themselves are imperfect, there are no perfect ways of measuring things, are there?
>
> Population Health Researcher (Public Health PhD)

Nine interviewees specifically discounted the likelihood of a single regression analysis providing a durable answer:

> That would be problematic, if people imagine they're going to plunge into a dataset and 'prove' conclusively, forever, that this is the 'right' answer.
>
> Geographer (Geography PhD)
>
> Regression models can be useful as a starting point, however, I can't imagine that regression models based on a few variables offer accurate representations of reality.
>
> Medical Doctor (Medicine PhD)

A similar sentiment was that the coefficients 'help' researchers arrive to at their conclusions:

> They represent help. I think they represent help to answer my questions. So, they represent possible effects of exposures on the outcomes. […]
>
> TC: So, they help you to answer the question, they are not themselves the answer to the question?
>
> The coefficients themselves? No.
>
> Biostatistician A (Biostatistics PhD)

This view of coefficients as being 'helpful' represents another route to the conclusion that it is researchers who answer questions and assign meaning, not models. The view of regression coefficients as contributing to an answer (rather than themselves being answers) implies that regression models do not fully capture or accurately reflect reality. Ten interviewees expressed this view, suggesting variously that regression coefficients approximate true relationships:

> I view it as **an approximation** to lots of these different relationships
> Health Economist E (Economics PhD)

Reflect some part or component of true relationships:

> I think that it's **one piece of reality.**
> Professor of Health Outcomes (Epidemiology PhD)

> It's always going to be a **partial representation of truth**
> Population Health Researcher (Public Health PhD)

That regression coefficients approach true relationships from a certain direction (implying that there are other directions):

> I think they are a depiction of **some view of reality**
> Epidemiologist A (Epidemiology PhD)

Or are inherently limited in the completeness of the picture they provide:

> They will **never fully describe reality**
> Health Behaviour Researcher (Health Behaviour PhD)

Therefore, for both interviewees who view regression as discovery, and those who view it as construction, results are not necessarily definitive or conclusive. An obvious next question relates to how interviewees navigate this uncertainty and draw conclusions from research data; but first, a related discussion surrounds the extent to which data can 'speak for itself'.

Several interviewees used language implying that data can communicate. The metaphor of data 'talking' or 'saying' was employed by interviewees from a range of disciplines and network clusters, and one interviewee framed their entire approach to statistical inference explicitly in these terms:

> It's just like interviewing someone**.** You ask them a question and they give you an answer, and sometimes you need to understand how history and personality and situation is going to condition the answer they give you. […] my approach to it [statistics] is to see it as a bit like interviewing the data.
>
> Public Health researcher (Geography PhD)

The presentation of researchers and data as being in a dialogic exchange merits close analysis, as on the subject of data 'talking', some tension was evident. On one hand, being attentive and 'listening' to data was presented as a virtue:

> When you confront data, it can tell you things you didn't expect. Are you listening?
>
> Social Epidemiologist B (Epidemiology PhD)

> The conclusion, whatever you want to call it, has to be based on what the data says.
>
> Cancer Researcher (Epidemiology PhD)

But, some interviewees identified risks associated with listening uncritically to data:

> Most of the time you can't let the data just speak, you need to put structure on it. […] what you get out of the model depends on how you set it up. I think for most things, that is true.
>
> Professor of Epidemiology (Medical Doctor)

This extract points to the central issue with the dialogic metaphor of statistical analysis. Data only 'speak' in ways facilitated by researchers; models do not build themselves, tables do not populate themselves, graphs do not design themselves (Barnes, 1982). Just as the image-makers of the 18th and 19th century were disappointed to discover that X-ray depends on expert operation and interpretation (Daston & Galison, 1992), regression models ultimately depend on researchers for their design, execution and interpretation. However, in the context of negative commentary surrounding the intrusion of the researcher into the inferential procedure, the appeal of data-as-talker may be that it frames the researcher in a passive manner, as a mechanical recorder of what data says rather than as an active mediator of what data can say.

A contrasting metaphor, employed while discussing the use of regression methods, framed the researcher in a more active light. Eight interviewees described their actions in intrepid terms, "exploring" relationships, data, and ideas:

> Most of us want to extrapolate to the human race. And when we do that,

> that is a matter of construction, constructing something empirically grounded […] But **when you're in your data**, and you're trying to establish, **it is more exploring what is there.**
>
> Health Equity Researcher (Sociology PhD)

This view of data-as-terrain presents the researcher as being active, determining both what is explored and to what extent. This view of data was more commonly expressed by statisticians, or interviewees with experience and confidence conducting analysis, perhaps suggesting that experience applying statistical methods shapes understanding of how data enters the epistemology. The tension between these views of data might be explored in detail, in a future research project.

9.3.3.3 'It depends'

For most interviewees, unless studying relationships already understood to be 'real', regression coefficients do not appear to be viewed as a final, complete and authoritative source of knowledge. Researchers must therefore look elsewhere for a basis on which to promote statements suggested by regression models to generalisable facts. Interviewees reported looking in a variety of places for confirmation that a particular statistical result has meaning (or does not have meaning), and these are presented in Table 13

It depends on… [n]	
How the model is setup [4]	We make decisions in model building, we make decisions further upstream in what we ask, the tools we use, how we manage data, how we code data, there are choices that are subjective. Health Education & Behaviour researcher (Health Promotion PhD)

Study Design [3]	It depends on the research design. If I participated in a randomised control trial I would feel differently about this than when I do a quasi-experiment, compared to doing a descriptive paper. Health Economist D (Economics PhD)
Research Question [5]	It partly depends on the model, and perhaps the questions you're trying to ask with it. Public Health researcher (Geography PhD)
Data Quality & Availability [5]	If you do use statistical models […] the underlying data need to be good. Public Health researcher (Public Health PhD)

Table 13: Places interviewees looked for confirmation that a statistical result has meaning

The contents of Table 13 represent source material for both positive and negative modalities which may be attached to individual regression coefficients. If a study's design is perceived as good, the data as being of high quality, and research question as appropriate for investigation via regression, interviewees described themselves as being more likely to trust the coefficient. However, any single item in Table 13 failing to meet expectations could lead to a result being dismissed. This perhaps explains why interviewees generally assumed there is some reason why a regression coefficient does *not* perfectly reflect reality, as most interviewees stressed it was only in rare, special cases that the full range of factors in Table 13 were satisfied.

9.3.3.4 The Eye of the Beholder

From the data presented, it appears that, despite the mechanistic, algorithmic origins of regression coefficients, interviewees do not generally understand them as being uncomplicated representations of reality, as being the 'words of nature'. Repeatedly, and in various ways, interviewees foregrounded the role of the human analyst; the interpretation of the coefficient might depend on what the researcher expects to find, might be understood as helping the researcher arrive at their own answer, or might depend on myriad other factors under the researchers' control. In addition, the coefficients themselves are influenced by the researcher's judgement during the model building process. It appears that the meaning of an individual regression coefficient lies in the eye of the beholder, it is the researcher, and their readers, who decide its meaning.

9.4 Significance, Meaning and Thinking

As was mentioned at the beginning of this chapter, the Null Hypothesis Significance Testing (NHST) framework represents a mechanistic approach to statistical inference. It represents an attempt to evade Duhem's paradox, by providing formal, rule-based guidance for deciding whether a particular statistical result has achieved 'Statistical Significance' (which, in some contexts, is synonymous with 'meaning'). The test involves checking whether the value of a particular statistic, the p-value, exceeds a threshold set by the researcher before the analysis is conducted.

9.4.1 P-Values

Broadly, the p-value represents the probability of the observed result, assuming there is no underlying difference in the data[18]. Therefore, a low p-value is desirable for demonstrating difference, suggesting the observed result is unlikely in the absence of underlying difference.

In some scientific spaces, the use of p-values reduces statistical inference to a yes/no question: if the p-value exceeds 0.05, the result is 'statistically significant', if not, the result is 'not statistically significant' (and, depending on the research area, may be unpublishable, see Head et al., 2015). This rule-based approach nominally removes subject area expertise and statistical expertise from statistical inference, limiting the role of the researcher and their judgement in the interpretation of results. The use of p-values to determine statistical significance appears to represent a

[18] Said another way, if a study is aiming to detect a difference in a health outcome between two income groups, the p-value reflects the likelihood of observing the study result, assuming there is no difference (in health) between the groups.

mechanical or procedural safeguard against the ills of expert judgement and interpretation of the type Daston & Galison (1992) described from historical sources. The moral dimension of this privileging of mechanical objectivity above expert knowledge in statistical inference has also been the subject of comment for several decades in medical research:

> “What used to be called judgment is now called prejudice, and what used to be called prejudice is now called a null hypothesis.” (Edwards, 1972: p180)

The continued use of the p-value as the only criterion upon which results are judged remains the subject of continued outcry (Amrhein et al., 2019), however, to date, most commentary has come from statistical associations (e.g., Wasserstein & Lazar, 2016) and prominent statisticians (e.g. Gelman, 2016). This PhD project provided an opportunity to hear from researchers who may not be engaging in the conversation. 32 interviewees (out of 43) mentioned or were asked to comment on the use of p-values within HIDR (I did not ask interviewees with limited time available for the interview, or interviewees who responded to my initial enquiry about their use of statistical methods in a way which indicated this was not a comfortable topic). Going into my interviews, I believed that all quantitative researchers who study health were actively wrestling with this issue. However, I quickly realised that while some interviewees leapt at the chance to express views on the topic, and had clearly followed debates closely, others were less aware. The following two extracts are illustrative of this contrast:

> It is a disaster, p-values, an absolute disaster. […] It is the greatest problem in our science.
>
> Social Epidemiologist (Epidemiology PhD)

> TC: There’s a bit of noise around, about P-values.
>
> Is there? I am not surprised.
>
> Geographer (Geography PhD)

I got the sense that for many interviewees the debate surrounding the use of p-values is occurring somewhere in the intellectual distance. Few interviewees discussed the issue in relation to their own research, or positioned themselves as stakeholders in the debate. Many had the sense that others would resolve the question:

> There are people debating it much more actively than me […] I respect the debate. I'm aware of the debate and I think they have some good points but **as long as they're sorting it out I'm happy to let them do it.**
>
> Public Health researcher & Medical Doctor (Medicine PhD)

Four interviewees commented on how long the issue has been the topic of debate and discussion within public health and epidemiology:

> It has been about forty years […] I think it is mind-boggling that this debate is still going on.
>
> Medical Doctor (Medicine PhD)

This seemed to contribute to a sense that entering the debate actively was of little likely benefit. Nevertheless, discussing p-values proved to be a window into more general attitudes regarding statistical inference. As with regression models, the judgement of the analyst and the importance of 'common-sense' emerged as central, and the role of the researcher, their expectations and their choices was repeatedly emphasised.

9.4.2 Thinking and "Stupidity"

Calculating Without Thinking

Nine interviewees said they felt that the use of p-values as binary decision-making tools discourages 'thinking' (nuanced interpretation of results). As discussed in Appendix I, developing and maintaining the kind of statistical literacy necessary for engaging quantitative data is challenging for some researchers. In this context, it is perhaps unsurprising that the p-value emerges as an attractive alternative, as it can be quickly computed and interpreted without reference to statistical theory, or subject-area expertise. Put another way, the p-value has the appearance of being mechanically objective. This aspect was presented in opposition to researchers' own thinking:

> To me, the worst thing is to transform something that needs to be *thought* and *discussed* into something which systematically is used as the solution,

> the ‘objective’ solution. […] It’s like a recipe, people use it ,maybe unconsciously, as a recipe. […] People need to think!
>
> Public Health researcher & Medical Doctor (Social Science PhD)

> I think it is a great loss of detailed thinking, it simplifies the thinking, hypothesis testing, young students are learning to reduce their thinking to a yes/no, and not looking at the data in detail.
>
> Social Epidemiologist & Medical Doctor (Epidemiology PhD)

This perceived lack of thinking was positioned as giving rise to blind application of the rule that only results achieving $p<0.05$ have meaning. In the next section I present the alternatives presented by interviewees to this rule-based approach.

Integrative approaches to statistical inference

Ideas about the appropriate interpretation of results failing to attain statistical significance (where $p>0.05$) were varied, and suggested two distinct approaches to statistical inference. Two interviewees (trained in psychology and clinical epidemiology) appeared to adopt the rule-based, binary interpretation of p-values wholesale:

> If it’s more than [p=]0.10, you don’t even look at it.
>
> Health Policy Researcher (Psychology PhD)

> One of my pet peeves is where the odds ratio is greater than one […] the p-value is not significant, but people will say “That’s an association” and I’m like, “No, it’s not!” That happens, I’m sure you’ve seen that.
>
> Cancer Researcher (Epidemiology PhD)

However, six interviewees (three from Cluster 2) advocated a different approach, and argued that meaningful, important findings may fail to achieve $p<0.05$:

> The significance thing, that gets to me. Not understanding, suggestions that it [the study] has failed because it isn’t significant. **It’s not that nuanced, the understanding of what that means.** […] someone who doesn’t understand those issues reads [p>0.05] as “it has no effect”.
>
> Health Economist E (Economics PhD)

> **You need to think.** […] This, deciding a p-value of 5% or 1%, 0.1% and then saying 'all on one side is true and everything on the other side is untrue' - **That is, of course, stupidity.**
>
> Health Equity Researcher (Sociology PhD)

If what seems like common-sense to one researcher seems like unthinking stupidity to another then this is an interesting state of affairs. It appears that at least some researchers using the same statistical framework are interpreting their results in different ways, corresponding to two, specific, frequentist[19] approaches to inference: a rule-based approach which refers only to statistical information from within the statistical test, and an integrative approach, drawing on sources of evidence and knowledge from outside. Some researchers appear to pursue (and believe they can obtain) mechanically objective statistical accounts of meaning, whereas others pass mechanical estimates through the prism of their own judgement.

9.4.3 "Significance"

The concept of statistical significance was quite widely criticised, but criticism was not uniform. Repeatedly, statistical significance was positioned as being arbitrary, or without meaning:

> P-values are dependent, enormously, on sample size. So you can have a low p-value for a microscopic difference in a humungous sample. Is it meaningful?
>
> Social Epidemiologist B (Epidemiology PhD)
>
> Statistical significance is kind of arbitrary, and not very meaningful.
>
> Epidemioligst C (Epidemiology PhD)

Almost all interviewees reported wanting to demonstrate something beyond statistical significance, but precisely what needs to be demonstrated seemed to vary across disciplinary backgrounds. Ten interviewees specified the particular 'significance' they seek in preference to statistical significance, presented below.

[19] As distinct from Bayesian approaches

Social & Sociological significance

The desire to demonstrate social significance was expressed by three interviewees with social science training, located in network Clusters 1 and 4.

> My standard approach is […] it doesn't matter so much whether it is statistically significant, it needs to be **sociologically significant.**
>
> Health Equity Researcher (Sociology PhD)

> [Name] told me once that there is a difference between statistically significant and clinically significant**.** And by clinically, [s/he] means **socially significant,** for public health.
>
> Public Health researcher (Social Science PhD)

Human Significance

One social scientists and one behavioural scientist referred to human significance:

> We have played around with the term 'humanly important'. We're not in clinical epidemiology any more. We haven't really found a better term for clinically important. But, [what we mean is] meaningful for population health.
>
> Epidemioligst C (Epidemiology PhD)

'Clinical Significance'

Three interviewees expressed a preference for clinical significance:

> It might be statistically significant but **is it clinically meaningful?** Does anybody give a toss about [it], is this going to change someone's life?
>
> Public Health researcher (Geography PhD)

'Biological Significance'

Two interviewees, both with medical training, mentioned biological significance:

> Both p-values and confidence intervals can be useful. They should be provided together. Both need to be interpreted considering their strengths, limitations, and **likely biological significance**.
>
> Medical Doctor (Medicine PhD)

Practical Significance

Only one interviewee mentioned ‘practical significance’, but this seemed to connect with a widespread desire that researchers focus on important questions (explored in the next chapter):

> The question comes down to statistical significance and **practical significance.** […] If you come out and find that low-income people have a statistically significantly lower likelihood of eating lettuce […] and you can’t do anything about it anyway, why bother?
>
> Health Policy Researcher (Psychology PhD)

Clearly, there is not a single term which describes the kind of ‘significance’ all researchers seek. These various significances map quite neatly onto the different types of knowledge researchers aim to produce (discussed in Chaper 7). Efforts to move away from statistical significance are generally understood to have been unsuccessful (Amrhein et al., 2019; Matthews et al., 2017), possibly because an important, yet under-recognised characteristic of statistical significance is that it serves as a standard which cuts across multiple disciplinary boundaries.

9.5 Conclusion

It is a well-accepted finding within SSK and STS that the interpretation of experimental results is significantly more complicated than eureka-style discoveries, or scientists ‘reasoning through’ their findings. This investigation of statistical practice within HIDR suggests that, despite their mechanistic origins, the interpretation of regression coefficients depends heavily on the analyst; their expectations, their suspicions, their own ‘lens of common-sense’. Negative modalities seem to attach to statistics, and function to limit the extent to which they are understood to correspond with reality. Findings from this chapter also echoed and reinforced key findings from Chapter 7, providing a more detailed sense of the way the various knowledges within HIDR (including negative knowledge) are pursued and generated. The ideal of mechanical objectivity was rejected by almost all interviewees, suggesting that the challenge of statistical inference cuts across disciplinary boundaries, and also that this challenge unites researchers from disparate backgrounds. This quality may explain why the field has (and many other fields have) been unable to arrive at a satisfactory replacement for the much-maligned 'Statistical Significance'. Together, these results strengthen my view that widespread changes in statistical practice will not be achieved without a picture of the social forces sustaining current practice. A mature sociology of biostatistical practice could generate such a picture, and would also contribute usefully to understanding both how and why statistical practice varies across disciplines.

Chapter 10: Questioning & Collaborating: The intersecting challenges of interdisciplinary research

10.1 Introduction

In the preceding chapters I outlined substantive differences in the way researchers of different disciplinary backgrounds approach and evaluate research about health equity, and sketched the epistemological drivers of such difference. In this final empirical chapter, I examine how these findings play out in interdisciplinary settings via analysis of interviewees' accounts of multidisciplinary, interdisciplinary and other collaborative experiences.

10.2 Interdisciplinary Research: Balancing Questions & Methods

Research projects are interdisciplinary (or cross/mono/trans-disciplinary) due to the perspectives brought to bear on a question, or set of questions. A surface-level analysis could show that researchers from different disciplines tend to ask different research questions, but, the preceding results chapters shed light on *why* they ask different questions, and suggest specific challenges which may arise when researchers work across disciplinary boundaries. Question-asking has a prominent place in the linguistics and science-education literatures (Bromberger, 1992; Ram, 1991), but is rarely the special focus of analysis in health-related research (Thagard, 2018). For this reason, before discussing interviewees' interdisciplinary experiences, I present some data illustrating the central role of questioning in HIDR.

10.2.1 The importance of the question

Questions emerged as entities of major epistemological importance in interviews, across disciplines. The interplay between questions and methods (or rather, the desire to elevate questions *above* methods) was a point heavily laboured by interviewees from a variety of backgrounds, in a range of geographic and institutional contexts. In total, twenty researchers specifically mentioned the importance of 'the question'. Of these, nine mentioned that good empirical research asks a good question, or an important question:

> TC: What are the hallmarks of good empirical work?
>
> I think it all starts with a very good question. That is more important than the methods. I think there should be more emphasis on what exactly are we studying? Why is this interesting?
>
> Health Economist D (Economics PhD)

It seems unremarkable that good research arises from good questions. However, two key issues are apparent even from the short quotation above. First, commentary on the value of 'good' questions implies the existence, perhaps even dominance, of 'bad' questions. Second, many interviewees did as the interviewee above, and stressed the relative importance and superiority of research questions *over research methods*, suggesting some tension between these elements of research.

10.2.2 Tension Between Methods & Questions

Commentary on question-asking frequently took the form of statements about interviewees' own practice. These statements had four general forms, outlined in Table 14.

Statement Form	N	Example
My research starts with the question, I don't start with the method	7	What drives me in my research is that it is the question that matters, and what is, sort of, the right toolkit to interrogate and answer that question? Health Equity Researcher (Epidemiology PhD)
I'm flexible about methods if they are right for my question	7	I don't really care. I'm not a purist. Some people say "it's only qual" or "it's only quant" [but] I am much more relaxed. Public Health researcher (Social Science PhD)
My research topic/question shapes my identity as an academic, not the methods I use	6	I have quantitative, qualitative work, all focused on one particular set of questions around [blank] and health inequalities Population Health Researcher (Public Health PhD)

My role as a senior researcher is to shape the research question, my team brings expertise about methods	3	The point for me is teams with complimentary expertise in order to be able to answer the questions, which may be motivated by the lead investigator […], but they have the [methodological] expertise. Social Epidemiologist B (Epidemiology PhD)

Table 14: Commentary on question-asking

The apparent tension between questions and methods seemed to originate from the impossibility of pursuing of a research program motivated *purely* by research questions. Considerations of what was logistically feasible, fundable, and publishable contributed to this difficulty, but the professional space and time to ponder questions of interest was also positioned as a luxury to which most interviewees aspired, but few enjoyed. As Barnes (1982) noted, the 'ship of reason' does not "power its own way through a silent sea of social contingencies" (p.117). Conditions are choppy, and both tactical and strategic factors must be weighed and prioritised for a project to make landfall. While idealised, linear models of science present all research as beginning with a question, in practice the research question is one consideration among many. Interview data suggested that, within HIDR, research questions are actively constrained by disciplinary norms and traditions, at least in some disciplines.

Each disciplinary community strikes a particular balance between valuing knowledge, valuing theory and valuing methods (Knorr-Cetina, 1999) and this appeared to inform disciplinary ideas about research quality, which in turn shapes what questions are considered fundable and publishable. For example, when methodology is highly valued, it can become the starting point and motivation for research, rather than being subservient to a question:

> Sometimes people have a method and they go looking for something to research with that method. […] That happens quite a lot.
>
> TC: Are you talking about any particular disciplines?
>
> Epidemiology, epidemiology and risk factors.
>
> Public Health Researcher (Social Science PhD)

As was discussed in Chapter 7, the status of epidemiology journals within public health amplifies the importance of epidemiological standards for research conduct, and was reported as preventing some researchers from pursuing questions which cannot be addressed via epidemiological methods. Also discussed was the way the focus on interventions within medicine and epidemiology shaped some interviewees' research priorities, both in the US and UK. If the dominant epistemology prioritises methodological rigour (and particular methods) above other features of research, studies failing to meet those standards may be discarded, or suffer in interdisciplinary contexts - even where they ask 'good' questions.

As was illustrated in Chapter 7, epidemiology and economics place high value on methodological rigour, and 'rigour' has a rather narrow definition within these strongly-classified disciplines. This was positioned as potentially incentivising against studies which ask good questions by researchers whose work leaned towards the social sciences:

> I do think that unlike any other discipline, epidemiology for example is focused mainly on method. […] Having the best methods, the best technical solutions, is the gold, it's high status […] Whereas I think that in the social sciences, having good theory is the higher status thing.
>
> Population Health Researcher (Public Health PhD)

As was mentioned briefly in Chapter 7, the Kuhnian model does not anticipate the long-term coexistence of multiple disciplines within a single research domain. Also not anticipated by Kuhn is the manner in which research questions are constrained by meso- and macro-level forces shaping the professional academic landscape (such as funding and higher education policy), or by publishing requirements in different disciplines. Interviewees from diverse backgrounds reported that top journals in epidemiology and economics value work which is novel, and that this intense focus on novelty is squeezing out research using established methods to address interesting questions:

> In economics you get more reward for doing clever things, elegant design or

> a new design, a new method, rather than applying existing methods to an interesting question.
>
> Health Economist E (Economics PhD)

However, perhaps confusingly, it was also widely reported that many published papers do not contribute in any novel way:

> TC: Are there papers that are not contributing much that is new?
>
> Absolutely. Almost nothing, I would say. […] [I see] big pieces of research that are limited, very limited, or sometimes completely useless.
>
> Public Health researcher & Medical Doctor (Social Science PhD)

There seemed to be a disconnect between what journal editors or funders considered 'novel' and what interviewees, specialists in the study of health equity, viewed as representing an important advance:

> I was in a meeting with [blank] who are funding [blank] a couple of months ago, and I was just fuming at the end of it, because every other word they said was "new" or "novel", "new and novel". They didn't seem to care what it was, they just wanted something they hadn't seen before. […] I'm not interested in novelty for the sake of it, I'm interested in something that takes us somewhere, moves things forward.
>
> Public Health researcher (Geography PhD)

In light of the previous chapters it is not surprising that there are diverse views about what 'moves things forward'. Very generally, researchers working with a critical or socio-cultural lens were roundly dissatisfied with studies that seemed to be asking 'old' questions applied to a new dataset, a bigger dataset, or via a new method.

> What definitely *isn't* an improvement is looking at the same boring association with more and more sophisticated ways of trying to say are they causal. I have no time for that whatsoever, I find it anaesthetising. Now we have birth weight and blood pressure, or something like that, we've been doing something on birth weight and blood pressure for 30 years! Now we're doing it with a structural something model. Jesus Christ.
>
> Medical Sociologist (Social Science PhD)

Another interviewee framed this frustration in Kuhnian terms:

> It frustrates me to see people reinventing the wheel and doing the same studies over and over again. And I guess, if you take a sort of scientific revolution approach to this, we are in the period of normal science and it gets kind of boring.
>
> Epidemiologist (Anthropology PhD)

There was a sense among some interviewees that the focus of funders and journals on methodological novelty has successfully driven innovation in methods, but that this occurred at the expense of innovation in questioning:

> This lack of new big ideas [in health equity research] is the result of the fact that we spend too little time sitting down thinking “What is really the interesting question here, what is really the thing we need to put our minds to?” Instead there is fashion and trends, very often driven by new, or pseudo-new, statistical techniques.
>
> Health Equity Researcher (Sociology PhD)

Surprisingly, this was a point upon which most economics and sociologists were in agreement:

> I think there should be more emphasis on what exactly are we studying? Why is this interesting? If you have a very cool identification strategy that might really help to get your paper published, well, I can see there might be benefits to that, but sometimes we forget what the interesting questions are.
>
> Health Economist D (Economics PhD)

However, this thesis highlights the ways in which the kind of insight viewed as ‘interesting’ varies across disciplinary groups.

In epidemiology and economics, incentives for researchers to improve the methodological sophistication of research were described as being stronger than incentives for tackling research questions broadly considered important, and interviewees reported following these trends themselves:

> We have fairly weak mechanisms for determining what the important questions are, and getting academic communities in collaboration with others to align what they’re doing to address those questions. I think we have persistent trouble with that.
>
> TC: Right, is it ‘I’ve got this hammer and I’m going to whack all the nails within my reach’? Is it that kind of thinking?

> Yeah. [Pause]. You know, I think I'm the same (laughing). It's not a criticism that wouldn't apply to me.
>
> Public Health Researcher (Health Services Research PhD)

With a clearer sense of the importance of good questions, and the challenges researchers face in pursuing good questions, I turn to interviewees' discussions of collaborative and interdisciplinary research in the next section.

10.3 Collaborative & Interdisciplinary Research in HIDR

10.3.1 Perceived Benefits of Interdisciplinary Research

With the exception of two interviewees,[20] it was unanimously felt that diverse disciplinary perspectives generate a productive tension, beneficial for individual scholars and for HIDR generally.

> The more perspectives you get in, even the more perspectives that would disagree with you, the better. You need to feel the resistance in order to feel certain about what you do. […] The more people disagree, the larger the potential for learning something new. Staying in one group where we all agree with each other is not very healthy.
>
> Sociologist (Sociology PhD)

All interviewees reported inter/multi-disciplinary collaboration of some kind, ranging from "Everything I do" (n=3) to "not much" (n=2) with most somewhere in between. For some (mostly senior) interviewees, interdisciplinary work meant synthesising multiple literatures by themselves, or serving as the "glue" which holds various disciplines in a multi-disciplinary project together (discussed further in Section 10.5). Interviewees discussed a range of benefits and motivations for engaging in interdisciplinary or collaborative work, falling into two broad categories; personal benefits and motivations, and scholarly or scientific benefits.

[20] One interviewee expressed skepticism about the benefits of multiple perspectives, and one interviewee felt the challenges of combining perspectives outweighed the apparent benefits.

Interviewees tended to use the term 'interdisciplinary' to refer to any collaborative cross/inter/trans-disciplinary effort, and I use the term in the same way in these sections.

Personal value of collaborating with other disciplines

Many interviewees reported that they enjoyed being exposed to new perspectives, and found understanding and integrating new perspectives into their work a rewarding, exciting and a productive challenge.

Interdisciplinary projects presented an opportunity for some interviewees to collaborate with scholars whom they like, and respect:

> Through years of trial and error, I have worked with people, and I now have a good group of collaborators who I enjoy working with, and it's easy to work with, and fruitful, and intellectually stimulating. I continue to work with those people […] I am not particularly precious about a group or discipline or anything like that, but there are good people who are good to work with.
>
> Population Health Researcher (Public Health PhD)

Six interviewees mentioned that disciplinary background was less important than the character or reliability of collaborators. As the above interviewee captured succinctly, it is ‘good’ to work with ‘good people’. However, these interviewees also stressed that it takes many years to develop such strong (interdisciplinary) interpersonal links, and the definition of ‘good’ did vary somewhat across disciplinary groups. For example, only economists linked statistical literacy to status as a ‘good’ collaborator:

> Most of the time the best collaborators have good across the board knowledge of stats.
>
> Health Economist E (Economics PhD)

Two social scientists mentioned valuing particular collaborators for specific theoretical or conceptual viewpoints. One epidemiologist with sociology training commented:

> For example, [Blank], he has been working at [institution in the US], he has been very important for our group because he is so good conceptualising health and politics, politics and health [...] he has been very important for us.
>
> Public Health Researcher & Medical Doctor (Public Health PhD)

10.3.2 Scholarly Value

The perceived scientific or scholarly benefits of interdisciplinary work were primarily linked with research questions, but also intersected with career stage, and the type of knowledge valued within an interviewee's own discipline. Seven interviewees mentioned that they are unable to answer their research questions from within one discipline:

> Given the type of research I do, I need a team. I am not a toxicologist, I am not an epidemiologist, I am not a physician, I am not a statistician. I am a social scientist, and I can do social science really well. But if, in my social science, I am going to look at the effects of social structure and culture on the prevalence of [a particular condition], I better have someone who can measure [clinical biomarkers], and I can't do that.
>
> Occupational Health Researcher (Anthropology PhD)

These interviewees tended to be in later career stages, as marshalling a team of investigators is not an opportunity possible for most early-career researchers.

Seeking Methods, Questions, Efficiency & Power

Interviewees described engaging in interdisciplinary research for a variety of scientific or intellectual reasons. Four interviewees described the aim of their interdisciplinary efforts as bringing new methods into their discipline, to answer their research questions;

> I know that colleagues from the economics department are trying [a new method]. So I am keeping my eye open for how they do that, and how we could benefit from that, with our study.
>
> Epidemiologist B (Epidemiology PhD)

Six other interviewees valued interdisciplinarity for exposure to questions from other disciplines, which could be answered via their usual methods. This latter type of interdisciplinarity is the classic Kuhnian expansion of normal science, discovering the ‘familiar in the unfamiliar’. Very generally, whether interviewees were seeking to import questions or import methods seemed to reflect the balance between negative and positive knowledges. Interviewees from disciplines with a strong and clear picture of ‘rigour’ seemed less likely to view interdisciplinary work as a strategy for encountering new methods. In particular, economists were more likely to view interdisciplinary work as a strategy for discovering new questions appropriate for their own methods:

> For me, it is perhaps different for others, but for me that is the most difficult part [of research], what is a good question? We [economists] are in this Ivory Tower. If I am an obstetrician then I see the problems I face, on a daily basis. It might be very practical, [a] very precise [question]. Economics is much broader than obstetrics, I think that is why it is difficult for economists to come up with good questions.
>
> TC: It seems like interdisciplinary collaborations might be a way for you to access interesting questions?
>
> Yes, yes.
>
> Health Economist D (Economics PhD)

Similarly, discussed in Chapter 8 was the importance of hypothesis testing within epidemiology (and the way ‘theory’ tended to mean ‘hypothesis’ within that paradigm). It is perhaps therefore unsurprising that some clinical epidemiologists framed the value of interdisciplinary collaborations for perceived improvements in their hypotheses:

> The perspectives of people from a wide variety of research and practice backgrounds form a gestalt, for developing more reasonable hypotheses.
>
> Medical Doctor (Medicine PhD)

In previous chapters, social epidemiologists emerged as distinctive for the manner in which their work blends epistemologies. Four social epidemiologists positioned interdisciplinary work as a faster way of achieving the co-creation of knowledge:

> If you’re separate, in your own field and your own journals, you learn less

> from each other and the co-creation of knowledge is less quick than you would perhaps otherwise see.
>
> Public Health Researcher (Public Health PhD)

The extracts presented thus far illustrate that while researchers bring specialised knowledge and methods to collaborative projects, accompanying this knowledge and methods are the specialised goals and epistemic priorities of the intellectual niche from which researchers come. If the overriding aim of social epidemiology is to bridge epistemologies, interdisciplinary research may feel 'efficient'. However, a researcher from a different background may have a different goal, and a different view of the same project.

Not all benefits related specifically to the creation of knowledge. One interviewee from a non-Anglo-Saxon country mentioned that interdisciplinary collaboration was a strategy for accessing the power of established research groups located in the US and UK:

> Some of these groups, they are much more powerful than we are. They have more resources, more people. For us to advance ourselves it is important to be working with them.
>
> Public Health Researcher & Medical Doctor (Public Health PhD)

For this interviewee and others, 'advancing' meant opportunities to publish more, or in better journals. Peer-review and publishing appear in every section of this chapter, because the perceived need to publish in high-impact journals was more evident in discussion of interdisciplinary projects than in any other part of interviews.

As well as accessing power from different continents, interviewees discussed enrolling the power of *other disciplines*. Economists were described as having credibility and influence in policy spaces, and collaborating with an economist could help researchers' findings advance into such spaces:

> Working with them is really rewarding because they bring all that quantitative prowess to the question, and have traction in the economic circles that other social scientists don't, they just don't. They [economists] can speak the economic language, in terms of impact and engagement. So that has been brilliant.
>
> Health Equity Researcher (Epidemiology PhD)

This is the first reference to the value of speaking a disciplinary 'language', explored further in Section 10.5

10.3.3 Perceived Pre-requisites for interdisciplinary work

The benefits discussed in the previous section are only attained if collaboration is successful (if, as many interviewees put it, the project 'delivers'). Interviewees tended to discuss interdisciplinary work in terms of what they felt 'you need' in order to make it workable:

> In order to make transdisciplinary research you need a number of things. You need the view, you need the knowledge, you need the language, you need the resources, you need the time. Unless you have these kinds of things, you have it in your mind but there is no room for discussion.
>
> Public Health researcher & Medical Doctor (Social Science PhD)

This included personal requirements, and team requirements.

10.3.3.1 Personal requirements & appropriate career stage

Discussions of character or personality traits required for interdisciplinary work connect with existing studies highlighting the attitudes, orientations and temperament of successful interdisciplinary researchers (e.g., Fam et al., 2017). The particular attributes interviewees felt were important or necessary seem to reflect a complex web of professional, academic and disciplinary forces which collectively constitute the challenge of interdisciplinary research.

Three interviewees discussed the importance of dedicating time and energy toward understanding other disciplinary perspectives, connected to a need for epistemic humility:

> The minute you work with somebody from a different discipline you realise that you're the learner, you're the beginner. […] [Interdisciplinary work means] embracing and trying to understand as much as you can of the paradigms they work within.
>
> Sociologist (Sociology PhD)

Failure to do this resulted in projects which involved different perspectives but did not integrate perspectives, discussed in Section 10.5.4.

Four interviewees described *confidence* as a prerequisite, and this meant confidence in oneself (to ask questions, and possibly appear ignorant in meetings) and confidence in ones' discipline, which may be questioned or minimised as a part of the collaborative process:

> Having the experience to know you're going to be a bit challenged. People are going to say things that make you shudder with revulsion, and of course you will say things to them which make them shudder. Also having a bit of confidence that your disciplinary perspective has something valuable.
>
> But, also the confidence to understand that other peoples' disciplinary perspectives have value, too. It's alright for people to do things differently to you, and possibly better.
>
> Public Health Researcher (Geography PhD)

This confidence was broadly understood to be obtained via experience. Two interviewees specifically stressed experience as a pre-requisite for interdisciplinary collaboration, and three others described interdisciplinary projects as especially hard or risky for early career researchers (ECRs), and that they should not 'rush in' to interdisciplinary work. This was because an established research profile within a single discipline was widely perceived as important (even essential) for securing a stable academic career. Perhaps predictably, given results of previous chapters, interviewees from the strongly-classified fields of epidemiology and economics expressed this point most forcefully:

> We have to work in a more interdisciplinary way, but […] we must not mistakenly do non-disciplinary work. Because that is sometimes what happens […] students don't know anymore, is this economics? What is this? […] it is difficult to learn from, and this is not a very nice way of putting it, from 'non-disciplinary' people.[…] Because, in the end, the field is very

> often very specialised, even within health economics […] you have your niche. That is the only way of getting good publications out, by being an ultra-specialist on something.
>
> Health Economist C (Economics PhD)

The sense of a pervasive drive toward specialisation, and specialisation in turn being the path toward a secure career, was not limited to epidemiology and economics. The same point was made in various ways by interviewees from diverse disciplines and contexts, from Scandinavia to Australia:

> For me personally it has always been a pleasure to learn more and get a broader picture on the world […] But very often the logic in research councils is that you need, to be at the research frontier, to be the 'real' expert on a very tiny little thing. […] If you are evaluated against those standards you fall short, of course, you cannot have width and depth at the same time.
>
> Health Equity Researcher (Sociology PhD)

One interviewee offered this account of their own interdisciplinary experiences, rising up the ranks:

> TC: Is there a career stage element there? I'm imagining once you reach a particular career stage maybe you have more control and you can say no [to interdisciplinary projects]?
>
> Yeah, definitely. Early on it is almost like, the higher go, you reject the hard projects. […] You are more selective, you think "this one is difficult" and you pick easy things that are publishable, and potentially will have a big impact. The other things you leave. […]
>
> Early on it is harder […] people are thinking "Who are you? why are you asking this? Just do it, go away." But if you get more power then they listen to you more, and are more willing to engage in some of your creative ideas.
>
> Health Economist E (Economics PhD)

This last point relates to another attribute viewed as important for interdisciplinary success. Credibility was positioned by interviewees as an important pre-requisite for a good experience collaborating with other disciplines. Latour and Woolgar (1986) conclude that scientists, as well as producing knowledge, are engaged primarily in the production of personal credibility. As cycles of credit progress, researchers convert funding and materials (data, equipment), into claims, publications and more credibility (which facilitates access to further inputs, and the next cycle begins). This concept is quite widely applied, including one analysis of HIDR (Smith, 2014) however, the concept is most typically used to explain the *behaviour* of scientists, and to illustrate the self-amplifying nature of scientific success. However, as originally framed by Latour & Woolgar, the scientific cycle of credit is a statement about knowledge construction which connects some of the findings of this thesis.

Latour & Woolgar ultimately conclude that 'truth' in science is the set of statements viewed as too costly to contest. Triumph in the agonistic field, (the socio-cognitive arena within which new claims stand, fall, or fade away), is connected to this view of credibility, because the 'cost' of contesting a claim is directly proportional to the credibility of the claim-maker. This seemed to be what the interviewee above was suggesting; if a researcher is perceived as lacking credibility, the price of conflict in the agonistic field may be perceived to be very low by collaborators, and that researcher's ideas may be dismissed.

This is not to suggest that credibility is something researchers simply possess, or lack. Credibility takes different forms in different contexts, career stages, and disciplines, however, researchers with medical training reported to have (and were discussed by others as having) access to cross-cutting credibility in both research and policy settings. One medical doctor reflected on their success in such settings:

> I guess I've got a degree of credibility. I'm old, I'm male, I'm white. I've got a medical degree, lots of honours and all these things. It sounds awful, god, but certainly […] I have that credibility which other people might not have. When I say something, they're not going to dismiss me out of hand.
>
> Public Health researcher (Medicine PhD)

Discussions of credibility and confidence seem to be connected, in that they relate to an individuals' capacity to evade or endure what Lamont (2009) termed *Disciplinary Prejudice,* which might be conceptualised as partly determining the terrain of the agonistic field within which claims are contested. Chapter 7 contained some examples of interviewees actively dismissing work from other fields (work which pursued a different kind of knowledge, or knowledge for a different purpose) and so this is perhaps unsurprising. In Lamont's study, such prejudice was overcome only within environments engendering respect and trust, cultivated by a skilled panel chair. The view of interdisciplinary spaces as potentially demoralising (or damaging) to researchers without the resources to endure open epistemic conflict seems to suggest that such respectful and trusting conditions are hard to come by in HIDR (explored further in Section 10.5.2). However, this is only a partial explanation for interviewees' reported belief that interdisciplinary work is especially difficult for ECRs. The above extracts caution ECRs to be wary of engaging in interdisciplinarity too soon, not only because their confidence or credibility may be dashed, but because the viability of an entire career might be jeopardised by an early interdisciplinary misadventure, or by becoming a 'non-disciplinary' person.

Viewed one way, this is the paradigm exerting its force in the classic Kuhnian sense: research is governed by paradigms, with strict ideas about the form good research should take. In deviating from disciplinary norms, ECRs put themselves in a vulnerable position. However, this does not align with the findings of Chapter 7 regarding the high value placed on instrumental knowledge, and also seems to undermine the status of 'the question' as being held above all other considerations, for some interviewees. When accounting for their own mental model and methodological preferences, many interviewees (especially, though not only, epidemiologists) explained that their grand aim was to produce *useful* knowledge, and this was a key criterion when evaluating the quality of research publications. However, when discussing what is safe or advisable for ECRs, or for securing success in the modern academy, this instrumental focus seemed to disappear from the ideal strategy; which was to specialise, and publish, at all costs. Therefore, concern with what is publishable may override what researchers' own intuition (and/or training) suggests is important, or interesting. No interviewee described their own thinking in this way, but some did comment in a more general way:

> People are most worried about writing a paper which is published in some first-quartile journal than they may be […] [in] the originality, or the quality of the work.
>
> Public Health researcher & Medical Doctor (Social Science PhD)

Interview data has supplied two explanations for this concern with what is publishable. First, not all kinds of research are valued equally by journal editors (e.g., the perception that qualitative research cannot be published in the International Journal of Epidemiology), second, researchers do not have the time to engage in 'risky' projects, as one economist stated in the previous section.

These findings may help to explain the question-vs-methods tension introduced earlier in the chapter, as researchers face multiple competing priorities in attempting to generate publishable output.

10.3.3.2 Team Requirements

Good interdisciplinary teams were described as being united around a common objective or mission. However, producing such alignment depends on having a common language with which to discuss the objective, a major and widespread challenge (discussed in Section 10.5). Interviewees valued and enjoyed involvement with interdisciplinary teams whose members respected each other, and each others' perspectives. One interviewee described this collaborative 'sweet spot':

> When you get the sweet spot of interdisciplinary working there is nothing better, professionally.
>
> TC: What is the sweet spot?
>
> You know, when it's not about *my* disciplinary approach as the dominant one, or 'it really has to be this way and not that way'- in terms of different disciplinary perspectives. When there is collective ownership of the research question, and everyone is in it to answer the question. They're not in it to have their disciplinary answer to the question. When you get that it is

> amazing, but it's very hard.
>
> Health Equity Researcher (Epidemiology PhD)

Here, again, is the central importance of the research question. Here also is concern about disciplinary preference threatening to obscure or dominate the question. This concern was widespread, and manifested in comments about collaborators asserting the superiority of their own disciplinary approach, or failing to 'check their egos':

> What you can't be dealing with are people who play the superiority and inferiority game. I've been either lucky or selective […] I have never got into that bullying competition, obviously if that's a thing that's going to take place you're not going to be doing productive work, and not enough of it.
>
> Public Health researcher (Social Science PhD)

Discussions of interdisciplinary spaces as potential sites of conflict helped to draw out interviewees' perceptions of mono-disciplinary projects as (relatively) more comfortable, or safe. This perception cut across disciplines and geographic contexts.

> [There is a] safety that you feel once you're established in a field. Once you step out of it you become slightly uncomfortable.
>
> Sociologist (Sociology PhD)
>
> Working with people in your discipline there is a bit less of having to justify your existence, so to speak.
>
> Health Behaviour Researcher (Health Behaviour PhD)

This sense of comfort associated with working within (rather than across) paradigms seemed to be rooted in what could be assumed, or taken for granted in mono-disciplinary contexts:

> Working within one discipline there would have been more things we could take as read. In those [interdisciplinary] projects we had to spend a little bit of extra time planning what we were doing, [and] exchanging with each other about why we were doing it.
>
> Geographer (Geography PhD)

The additional effort and time required to undertake interdisciplinary work was presented by many interviewees as a barrier to such work, especially in the context of scarce research time, and an academy focused on the volume of published output.

10.4 Energy, Outputs & Language: The intersecting challenges of interdisciplinary research in HIDR

10.4.1 Science, fast and slow: Disciplinary Pace & Publication Culture

References to Disciplinary 'speed' or 'pace' appeared to act as shorthand for multiple findings in this thesis.

> It is sometimes frustrating because we work at different paces, we speak different languages, and you really have to allow time for people to interact.
>
> Epidemiologist (Anthropology PhD)

By these terms it was not meant that researchers work harder in certain disciplines and less-hard in others. Rather, the pursuit of certain kinds of knowledge, via certain methods, tends to take a certain amount of time. This amount of time (whatever it may be) feels natural or usual for members of a given discipline, and, when met with a different approach, the relative speed or slowness can itself feel jarring, or unscientific. Diverse disciplinary speeds manifest publicly in authors' publication counts, and this highly-visible disciplinary difference resulted in judgement, both from and toward scholars who publish a lot, or a little:

> The single-author monograph is the gold standard publication in sociology, or geography, or whatever it might be, and the expectation is that you publish one or two papers every two or three years, but they have to be substantial. Whereas the model in public health is, you know, 50 papers a year for some big group that publishes loads. So, somehow they [both] see it as weak scholarship […] I think we are stuck, both ways.
>
> Population Health Researcher (Public Health PhD)

Underneath perceptions that the speed of research varies were feelings that researchers from other fields are 'precious' or 'picky' about particular features of research such as the form and use of theory, or measurement. In addition, statistical differences between disciplines were noted, however, even collectively, discussion of these issues was dwarfed by discussion of the challenges associated with research output or publication, and language, which also intersected with institutional criteria for academic advancement.

Across disciplines, and countries, interviewees described academic institutions as being supportive of interdisciplinary efforts in a nominal or surface-level way, but generally felt that academic institutions functioned to limit interdisciplinary collaboration. One clinician in the US described their institution as 'seeming more interested in turf protection than building bridges'. One economist felt that there was no force in HIDR generally, or within their institution specifically, bringing researchers of diverse backgrounds together. Just as publication was central to the perceived risk of interdisciplinary work for ECRs (discussed in Section 10.4.2), the use of publication counts as criteria for evaluation and promotion within academic institutions seemed to act as a disincentive for interdisciplinary work for interviewees at later career stages, due to concern that interdisciplinary work is difficult to publish.

> In the long run I think it [interdisciplinary work] is a real strength, but […] our institutional structures I think make it more complicated.
>
> TC: What is it about the structure?
>
> The basis of assessment, the opportunities for publication I would say […] it can be a bit complicated to get that [interdisciplinary research] published in a way which can help you build your career.
>
> Geographer (Geography PhD)

Publishing work 'in a way which can help you build a career' meant adhering to disciplinary publication norms, or what some interviewees termed their 'publication culture'. This included the expected frequency of publication, but also the expected *venue* of publication, and expected *number of authors*. Deviating from all or any of these norms might result in a penalty:

> In principle it [interdisciplinary work] is something very good, but in practice it is something very difficult […] [due to] differences in publication culture. Even though I want to collaborate with this doctor, if I ask him for an opinion he thinks he should be a co-author because that is normal in his field. Which is perfectly fine with me, but then I get penalised for that because my university tells me 'we don't multiple authors'.
>
> Health Economist D (Economics PhD)

Interviewees described uncertainty regarding whether an interdisciplinary project would generate output, but also (if outputs did materialise) whether they would be perceived as valuable within their home institution, or discipline.

> When it gets to the publication stage, it is very often that it doesn't matter so much when whether you have published in The Lancet if you hoped for a promotion here [in the economics department]. And in the medical centre, there they don't take you seriously if you don't have 20 publications per year.
>
> There is a different publication culture. They have many more, shorter articles. In economics it takes 5 years to get a paper published and [for promotion] they only look at your 5 or 10 most important papers, not all the rest. It is a different publication and promotion culture.
>
> Health Economist C (Economics PhD)

These concerns were most intense (and most clearly illustrated via examples) by economists. This is unsurprising, as that discipline is so strongly classified with a relatively stable (and rigid) picture of scholarly excellence. However, concern regarding publication culture and the difficulty judging the quality of interdisciplinary work was not limited to economists, and the status of interdisciplinary research a being hard to evaluate and publish has been established in single-country studies of interdisciplinarity (Mansilla et al., 2006; Feller, 2006).

Publishing frequently in high-impact journals is not a trend which has spontaneously arisen within HIDR. Interviewees are responding to macro- and meso- forces in academia, which is increasingly quantified and increasingly lacking space for reflection, and deep-work (Smith, 2010). For this reason, I do not interpret widespread concern with publication and output as careerism, but rather a reflection of what is necessary to build and sustain an academic career in a health-related research field.

When submitting work to journals, interviewees from diverse disciplines described the value of work which reads as 'belonging' to a particular discipline, and papers which sat uneasily within a discipline were described as being difficult to publish:

> If you're trying to publish in the mainstream journals it's really difficult.
>
> TC: So the issue wasn't with the collaboration itself, it was with publishing the product of that collaboration?
>
> Exactly yeah, because it's not seen as one thing or another.
>
> Population Health Researcher (Public Health PhD)

This quotation echoes an extract earlier in the chapter, wherein ECRs were cautioned against becoming 'non-disciplinary people'. This seems to suggest that researchers and papers with ambiguous disciplinary identities should perhaps anticipate challenges in academia, as being an interdisciplinary entity might be functionally the same as being a *non*-disciplinary entity, and a disadvantage. In the context of an academy striving for efficiency (Archer, 2008), anything which adds time and complexity to the research process might be avoided rather than confronted, despite widespread sense that interdisciplinary work is important, and addresses good questions.

Some interviewees explained that they did not attempt to produce interdisciplinary outputs from interdisciplinary projects, due to journal word limits:

> We're also somewhat limited by what you can include in a paper, right? In the grant application you're including all of the different [parts of a project], you're giving a blueprint. But when you publish, you get one room: "Here's what I found in this room, I can't tell you what I found in the rest of the house because I don't have enough words, I don't have enough space, but in this room this is what we found".
>
> Health Education & Behaviour researcher (Health Promotion PhD)

It is telling that this interviewee did not discuss the possibility of submitting longer papers to alternative journals, but rather elects to present interdisciplinary projects as multiple, smaller pieces. In this way, work which is pitched and funded as an inter- or multi- disciplinary project may not produce any multi- or interdisciplinary outputs. This is described by Nowotny (2003, p.184) as the 'clumsy disaggregation' of interdisciplinary research in academia. The difficulty publishing interdisciplinary research has been reported in other studies, but - as interviewees themselves mentioned - the expected number of publications per year in health-related fields is unusually high, and the focus on peer-reviewed journal articles (rather than book chapters or conference papers) unusually narrow. This puts journal editors (gate-keepers to publication) in a powerful position to shape health-related research, including interdisciplinary research. As many journals are overtly disciplinary institutions, this is an important way that disciplinary paradigms actively shape the design and conduct of research in HIDR.

In the previous section, interviewees reported navigating difficulties arising due to variation in goals, norms, and traditions of knowledge production by finding 'good people' and/or by synthesising diverse literatures on their own. However, difficulties related to publishing interdisciplinary work cannot be resolved via similar strategies, and these difficulties were reported to dis-incentivise interdisciplinary research, especially for researchers working outside the epidemiological paradigm. In general, academic careers were discussed as being built on peer-reviewed papers, which were the main way interviewees demonstrate to their employers that they are allocating their time effectively:

> Academia is a business, the currency of which is grants and papers. You need to be publishing to show you're doing something. We do need to do that work. But, it is a bit frustrating when you see the work. […] I think we probably are asking the questions we need to ask, but we're not prepared to put down the things that aren't working. […] It's much easier to do a small intervention which shows some slight change in a process, or outcome. It is much harder to [stop, and] say 'what can we realistically do that would help us tackle population-level inequalities'?

> Public Health researcher (Geography PhD)

Interviewees from all disciplines and all network clusters discussed this last point, that doing work which cuts across disciplines and dramatically advances understanding is hard, and especially difficult in the face of an evaluative culture which signals quantity and methodological novelty as being important. If job-security depends on publishing regularly, it is less-risky to produce mono-disciplinary, descriptive work which recycles questions (or methods) than to produce interdisciplinary work wrestling with the fundamental drivers of inequity in a ground-breaking way.

10.4.2 Disciplinary dynamics: Core & Periphery

Overlaying challenges with finding collaborators to get along with, and challenges publishing the output of interdisciplinary research was the sense that some disciplines (especially social and behavioural sciences) were not highly-valued in multi and inter-disciplinary contexts. This was not attributed to personal disagreement, or ego, but rather to the hierarchy of disciplines apparent within population health research, and interviewees' sense that epidemiology and medicine were at the top of this hierarchy. One interviewee (very well-known within their discipline) described feeling as though collaborative projects tended to involve a 'core' and 'periphery', and that sociology was always relegated to the periphery:

> Basically there is a core discipline and then there is a periphery. And you can bet your bottom dollar you are not the core discipline.
>
> TC: Why? Why aren't you the core discipline?
>
> Because the core discipline will be something with statistical clout, and will have this strong individualistic focus. So if you come in with things that add complexity, and talk about much more of an experientially-based understanding - rather than something you can work deductively through statistical analysis - it is very difficult to position yourself at the core, or be recognised at the core.
>
> Sociologist (Sociology PhD)

This seem to directly connect to the findings presented in Chapter 7 and Chapter 8, to the kind of knowledge researchers value, and to researchers' epistemological styles. The above interviewee describes feeling as though approaches which witness lived experience, or preserve and explore complexity are not easily integrated into the positivist/utilitarian style, which is dominant. A different interviewee, on a different continent, with different training, made a similar comment:

> Within that public health community, the biomedical paradigm is the dominant paradigm. So, in terms of the hierarchy of knowledge within all of that, the hierarchy of disciplinary knowledge, the default is a biomedical explanation for health. […] In the field, those interests with most power I think are the medics, medical voices, so that is privileged compared to some of the other voices […] it comes down to what mental model gets privileged.
>
> Health Equity Researcher (Epidemiology PhD)

This core/periphery dynamic was also evident in descriptions of what interviewees termed interdisciplinary 'box ticking'; the involvement of scholars from diverse backgrounds on a project without intending to collaborate meaningfully. This phenomenon was described by interviewees in diverse geographic and institutional contexts, including economists, social scientists, and behavioural scientists who described feeling devalued in some interdisciplinary settings:

> Sometimes you don't click with other collaborators. […] At the end of the day they're trying to check some box saying "I needed this other discipline on my team" but not really valuing your contribution. I think that's part of the learning process, you find those collaborators who can really appreciate what *you* bring to the table
>
> Health Behaviour Researcher (Health Behaviour PhD)

A power imbalance is implicit in these metaphors. The 'boxes' do not belong to a team, they belong to collaborators at the 'core', and others are enrolled in order to provide a veneer of interdisciplinarity, for the benefit of those core investigators. The flexible definition of 'interdisciplinary research' (as encompassing cross- and multi-disciplinary projects which do not integrate disciplinary approaches) seemed to be behind this trend, creating a situation where a project presented on a grant application as "interdisciplinary" could proceed in a fragmented, disciplined manner:

> The researchers have said on paper [on the grant application] that they will collaborate with researchers from other disciplines, but in practice they are [each] doing a tiny thing that belongs to their discipline, and they pass that to someone else, and the project rolls on, without much intermingling.
>
> Health Economist E (Economics PhD)

This is the rhetorical function of 'interdisciplinary' suggested by Frodeman (2017), that the vague nature of the term “allow[s] academics to gesture toward conducting research that's more relevant than ‘normal’ disciplinary knowledge, while avoiding the painful task of actually working with people outside the academy" (p.4).

Without exception, in examples provided by interviewees, box-tickers were clinicians or epidemiologists. The superiority of epidemiology or position of epidemiology at the core of collaborative work was also asserted in the minimising or scaling-down of other (disciplinary) contributions to projects:

> There are some people who, they will be told that they need a health economist [on their project]. They will come to you and say “I need a health economist”, [but] actually, they don't care. They don't acknowledge what your discipline can bring. It means they want to scale your part down if they don't value it, or ignore it.
>
> Health Economist E (Economics PhD)

This experience of epidemiological or clinical collaborators scaling-down parts of projects was also described by two geographers:

> What I thought was the more interesting bit, the bit that I was really interested in, didn’t really happen because compromises were made. […] I was really interested in trying to bring a geography perspective. There was a lot of resistance to that, when it actually came to the doing of the work, it wasn’t seen as being a priority, and so it got a little bit side-lined.
>
> Health Geographer (Geography PhD)

Other interviewees felt themselves peripheral based on the timing of their invitation to participate in a project. Behavioural scientists described being brought onto projects at the conclusion of analysis, to assist with the communication of findings, but wished that they had the opportunity to inform research at the outset.

I do not view these data as reflecting a sinister intent among epidemiologists or clinicians, and interviewees also stressed this point:

> It's not necessarily done as a power play, to assert the supremacy of one discipline over another.
>
> TC: It's not a hegemony?
>
> (Laughing) No […] but in my experience, those are relatively few and far between, those genuinely interdisciplinary pieces.
>
> Sociologist (Sociology PhD)

So many similar experiences suggest there is something about health-related research (including HIDR) which results in non-biomedical approaches being systematically devalued or minimised. This finding connects with existing studies, wherein social scientists reported not feeling as though they are in the 'driving seat' when collaboration is led by natural scientists (Lyall, 2019) . Recalling Abbott's (2001) fractal distinction model of scientific culture, it perhaps should not be surprising that HIDR contains a natural/social science distinction, and that this distinction functions to minimise the social sciences, given that this distinction is a part of broader scientific culture.

My study cannot shed light on the mechanism by which this dynamic arises, manifests and operates in individual projects. However, discussion of the field in terms of core and periphery does suggest a new interpretation of the bibliometric network, wherein other disciplines exist at the edges of an epidemiological 'core'. The yellow zones highlighted in Figure 18, below, correspond with the groups of interviewees who felt as though they had been involved in a 'box-ticking' project. No interviewee in the blue area reported such an experience, and this region of the network does contain a high proportion of researchers with epidemiological and medical training (certainly higher than the yellow, peripheral zones).

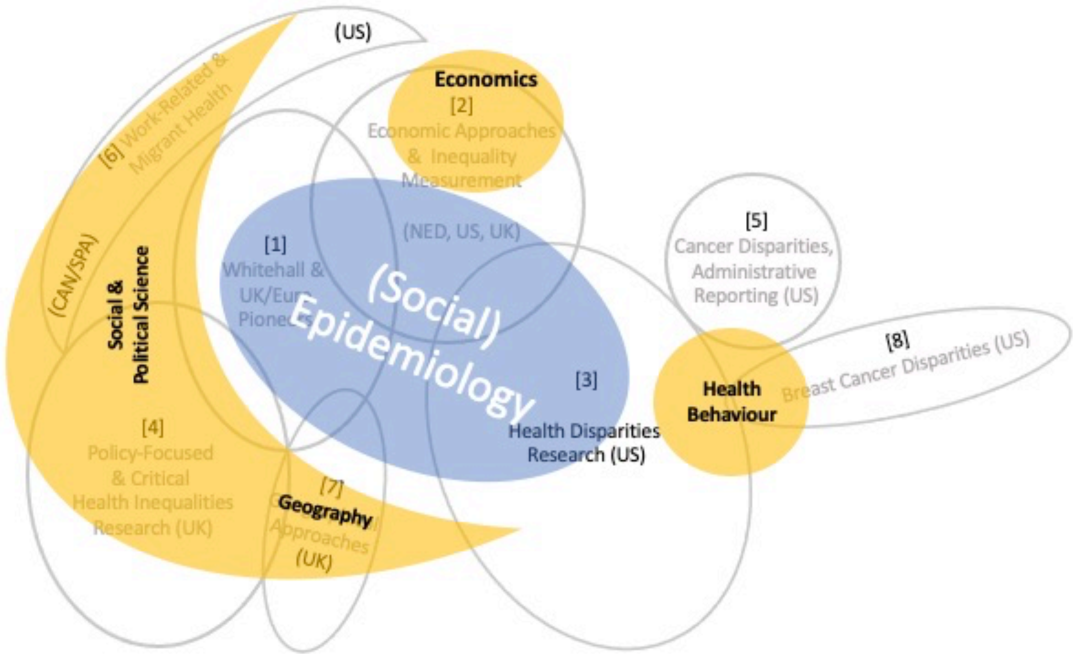

Figure 18: The HIDR bibliometric network in terms of disciplinary 'core' and 'periphery' as discussed by interviewees.

Given the challenges discussed so far, it is unsurprising that interviewees ultimately tend to collaborate with scholars they personally like. But, in practice, 'liking' or 'clicking with' collaborators seemed to mean agreeing on important points:

> The behavioural vs structural [determinants] […] most of the researchers I collaborate with would agree with me. I suppose that is why I collaborate with them (laughing).
>
> Public Health Researcher (Social Science PhD)

And to mean collaborators with whom interviewees can communicate:

> The other disciplines [I collaborate with] would mostly be people I can easily talk to. So epidemiologists, mostly.
>
> Health Economist D (Economics PhD)

Language, the most widely discussed challenge, is the subject of the next section.

10.5 Language, Possible Worlds & the True/False Game

Scientific knowledge is disseminated via technical vocabularies (Knorr-Cetina, 1999) posing a difficulty in interdisciplinary settings where members may not share such vocabularies. As was mentioned in the literature review, Kuhn (1970) argued that ‘complete’ communication isn’t possible across paradigms. In his later writings, Kuhn positions incommensurability as being a consequence of the ‘untranslatability’ of scientific concepts across paradigms (Kuhn, 2000a) and focuses analysis on the acquisition and deployment of scientific lexica:

> "To possess a lexicon, a structured vocabulary, is to have access to the varied set of worlds which that lexicon can be used to describe. Different lexicons - those of different cultures or different historical periods, for example - give access to different sets of possible worlds, largely but never entirely overlapping" (Kuhn, 2000a: p61)

This view of scientific language rests on a more general view of meaning as being rooted in a term’s use, rather than within the term itself. Under this view, technical definitions are of limited value, because “knowing what a word means is knowing how to use it *for communication with other members of the language community within which it is current*" *(*Kuhn, 2000a: p.62 emphasis added) The assumption that unrestricted scientific communication across paradigms is possible rests on the assumption that anything can be said in any language. Kuhn rejects this assumption, because a paradigmatic lexicon constrains the way the world can be described.

This feature of disciplinary epistemologies has been demonstrated in this thesis. For example, epidemiologists and economists tend to place high value upon abstracted, generalisable findings, and one consequence of this focus is that these disciplinary lexica do not contain terms and concepts permitting the discussion of complexity and context as they relate to (classical) epidemiological and economic research (recall one economists’ comment that studies of life-time relative disadvantage “do not exist”).

Kuhn describes the process of ‘enriching’ a lexicon by co-opting terms from another as being possible, with effort. Galison (1999) also describes such half-way languages, which evolve within what he terms the ‘trading zone’ of collaboration.

> "Two groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged; they may even disagree on the meaning of the exchange process itself. Nonetheless, the trading partners can hammer out a local coordination, despite vast global differences" (p.138)

Galison presents the 'pidgin', a simplified, restricted form of communication containing elements of two languages. Over time, pidgins may be extended to serve a wider range of purposes, perhaps ultimately expanding into a 'creole'.

The originating example of the trading zone described by Galison is the development of radar technology; a long-term, urgent and high-pressured collaboration between branches of physics. Even in this instance of neighbouring paradigms, participants needed to 'hammer out' a basis on which to coordinate, linguistically. Coordination between more distant paradigms such as medicine, economics and sociology might be expected to require yet more time and effort. To my knowledge, no creole has been documented within research about population health (they have been documented within biological art, and health-related commerce, see Leeper et al., 2010; Shin et al., 2018), however Galison's case-study provides an important example of a mechanism by which linguistic coordination across paradigms is possible.

However, the late-Kuhnian view of language sounds a note of caution, and anticipates a problem widely experienced by interviewees: Members of different paradigms may arrive at a half-way language to facilitate *local* coordination, however "the price of combining them [lexica] is incoherence in the description of phenomena to which either one might alone have been applied." (Kuhn, 1989: p.74). For this reason, the results of such a collaboration cannot be easily framed in mono-disciplinary terms, and results may not be recognised or understood by other members of the participating paradigms. This poses a problem if results are to be published in disciplinary journals.

Kuhn's position on language also helps to explain the sense some interviewees had that they were ‘talking past’ each other in debate. Kuhn’s view of debate between paradigms as futile stems from his position on disciplinary lexica, because “evaluation of a statement's truth value […] can be conducted only with a lexicon already in place, and it's outcome depends upon that lexicon” (*ibid*, p.77) Therefore, the prospect of ‘full’ communication is dim. If different lexica describe different worlds, there is no (semantically) neutral language into which all scientific terms, ideas and theories can be translated, and no neutral ground from which to choose between paradigmatic approaches. Therefore, there are no universal rules for what Kuhn terms (after Wittgenstein) the “true/false game” (*ibid,* p.100).

To take a recent example from health equity research, JP Mackenbach (Bibliometric network Cluster 1) recently published a commentary containing the following statement:

> There is surprisingly little robust evidence that the correlation between socioeconomic inequalities and health inequalities is causal, in the sense that socioeconomic (dis)advantage produces health (dis)advantage.
>
> (Mackenbach, 2020; p.615)

The ‘truth’ of this statement cannot be evaluated in a multi-disciplinary domain where there is no common understanding of the terms ‘robust evidence’ and ‘causal’. Results presented in Chapters 7 and 8 suggest these terms do not have a shared meaning across the field. Indeed, responding commentary from sociologist Olle Lundberg (2020) has focused on the meaning of these specific terms. It was with reference to such debates that one interviewee made the following comment about HIDR:

> I tend to think of the field less as a terrain of debate and more as a situation in which people talk past one another, which happens to a considerable degree. […] I don’t know whether there are debates, there are tensions.
>
> Health Policy Researcher (Social Science PhD)

The Kuhnian view of disciplinary language anticipates this absence of productive debate. In multi-disciplinary domains, certain statements may not even be *candidates* for being true or false in all disciplines, as this, too is lexicon-dependent. Chapter 8 included a quotation from a sociologist describing the ways in which ‘structure and agency [are] dynamic and interlocking processes’. This is an example of a statement which cannot be translated into all disciplinary lexica, and is not a candidate for being true or false in all disciplines involved in health equity scholarship. This reasoning leads Kuhn to conclude that, when it comes to cross-paradigm communication, “there is no such thing as being merely right or wrong” because most scientific statements are “located within an elaborate lexical and theoretical system" (p.83). It is for this reason, Kuhn argues, that disciplinary cross-talk tends to be circular, and may never resolve.

However, Kuhn concludes that understanding across paradigms is possible, occurring via the actions of *bilingual individuals*, not in the form of translations. He argues also that the lexical isolation of disciplines is not problematic, but is an essential precondition for development of knowledge, because “the process of specialisation, with its consequent limitation on communication and community, is inescapable." However, this is little comfort to interviewees, who described immense difficulty in communication when trying to execute interdisciplinary science. In this final section I review these data, with the Kuhnian lens just described.

10.5.1 Hard work & Magic Words; Diverse terms, diverse meanings

Twenty-five interviewees specifically mentioned that communicating across disciplinary lines is difficult, and/or impedes research progress. A dedicated chapter could have been written about the role of language and disciplinary lexica in collaborative HIDR, however an extended, detailed analysis is outside the scope of this general discussion. In this section, I focus on the most commonly-described challenges regarding use of language in interdisciplinary work, and draw out the links with the Kuhnian view of disciplinary lexica, with previous sections of this chapter, and previous chapters.

Overwhelmingly, interviewees described language and terminology as the site or subject of effort and hard work in interdisciplinary collaborations:

> There are differences across disciplines, people have to work hard to understand that.
>
> TC: What kind of differences have been important?
>
> Terminology.
>
> Cancer Researcher (Epidemiology PhD)

Interviewees located the source of this difficulty chiefly within the way meaning varies across disciplines. As one interviewee described, language has ‘baggage’:

> If you do work across lots of disciplines you become sensitised to the fact that when you hear somebody using a particular word it actually has baggage that comes with it, and usually is an indicator they have a particular paradigm. […] If you’re familiar enough you can decode things.
>
> Public Health researcher & Medical Doctor (Medicine PhD)

Getting familiar enough to decode things emerged as a (perhaps *the*) central task of interdisciplinary scholarship. Almost all interviewees described expending time and effort in pursuit of a common language with their collaborators, understood to be key to success:

> We have to find some common words which we can agree on to describe the problem and describe the opportunities. […] The common language exists to some extent, but people use those words differently.
>
> Health Education & Behaviour researcher (Health Promotion PhD)

However, no interviewee reported having arrived at a common language which was stable, and enduring. The reasons why a common language remains elusive fell into three categories, visualised below in the panels of Figure 19:[21] A) The challenge of disciplinary terms having diverse, existing meanings; B) the challenge of similar or identical concepts having diverse, existing terms or labels; and C) the challenge of concepts not existing in other disciplines, or instances of total communication breakdown.

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[21] This figure would perhaps work more effectively with examples from within HIDR, however, much depends on the lexica available to the reader (which is the very point illustrated by this figure)

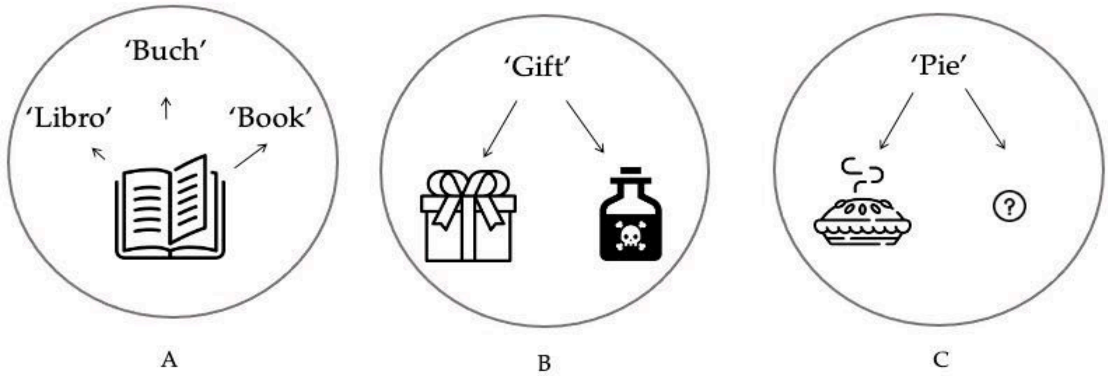


Figure 19: Three challenges with translating terms across disciplinary lexica. **A:** The same concept (book) may have diverse names in other lexica. **B:** The same term may have different meanings in different lexica ('gift' in German means 'poison'). **C:** A term may have no meaning in another lexica, and be untranslatable. (For example, the Dutch language has no single word equivalent to 'Pie' in English, encompassing both sweet and savoury pastries)

The first challenge (same term, different meanings) was best illustrated by the experience of geographers, for whom the terms 'space' and 'place' have particular meanings not shared by other disciplines. Communicating the precise meaning of these terms therefore required additional effort:

> If I'm talking about 'space, place and environments' that can be received in different ways than what I mean […] there is always a journey to go on there.
>
> Health Geographer (Geography PhD)

Geographers were well-practiced at presenting these core concepts in terms which were likely to be understood by others, a finding which aligns with previous studies describing geography as itself interdisciplinary (Skole, 2004). However, this was a necessity for all interviewees in the 'peripheral' (yellow) regions of Figure 18.

Therefore, in addition to being a learner (as one interviewee described in Section 10.3.2), interdisciplinary spaces may require researchers to be teachers. These data lead me to question whether and how the balance between teaching and learning roles in interdisciplinary spaces may shape research projects, intersect with career-stage dynamics, and/or shapes the power dynamics discussed by interviewees in Section 10.4.2. Mismatched assumptions about who enters collaboration to teach and who to learn may result in unsuccessful or unsatisfactory collaboration, or be a part of the mechanism via which certain disciplinary voices are privileged, or amplified. Fruitful educational exchanges between teachers and learners takes time, which many interviewees described as being in short supply. The 'sweet spot' one interviewee discussed might therefore refer to situations where every participant is both willing and able to teach, and to learn.

More common than the same term having diverse meanings was the same meaning having diverse terms (Panel B, Figure 19):

> Sometimes you find your ideas are similar but we use completely different names, different labels for the same things.
>
> Biostatistician A (Biostatistics PhD)

This is perhaps unsurprising, as so many disciplines present within HIDR are tackling quite similar questions. However, even when underlying concepts are similar this was described as creating work in collaboration, and being a source of interpersonal difficulty:

> I once had a debate in an interview with an economist who used different language for the same concepts we have in epidemiology. And I translated it back into the concepts we use in epidemiology, but he said "no, no" and went back to his language. You can have misunderstandings, if someone is not willing to step outside their own comfort zone and listen to different concepts for describing the same thing.
>
> Public Health Researcher (Public Health PhD)

Here is another example of an interviewee stressing the importance of leaving ones' comfort zone to engage with other disciplines, but further specifying that this includes being willing to employ terminology from other fields. The late-Kuhnian view of language comes into view, also: In the above reference to an unsuccessful attempt at translation, the interviewee reported that the other participant did not understand their translation as corresponding with their understanding of the 'same concept'. Seven other interviewees indicated that, where similar concepts have diverse names, attempts to translate between lexica did not seem to work in a straightforward way:

> Our language is sometimes different, really we're talking about the same thing and we don't sometimes know. […] I'm thinking of confounders vs endogenous/exogenous [in economics], we don't say that in epidemiology. I think that if I translate it we're talking about *some* of the same things, but [trails off] […] It gets confusing.
>
> Social Epidemiologist A (Epidemiology PhD)

This confusion in turn generates work, as time, energy and effort are required to 'hammer out' (in Galison's parlance) a locally-stable coordination with respect to these terms. Results presented in Chapter 7 suggest interviewees have subtly different understandings of health (as a research object). The confusion described by the interviewee above was reported to extend to concepts as fundamental as 'health':

> If, by health, we understand different things, it is very difficult to come to generalising statements regarding inequalities in health. Because everyone else has a different concept.
>
> Social Epidemiologist & Medical Doctor (Epidemiology PhD)

In the social sciences, such disagreements sometimes centred around collaborators' willingness (or unwillingness) to call a theory by an alternate name:

> I am always rolling my eyes, there is a jargon that sits in sociology, a jargon within political science, you're talking about the same things but its someone else's theory. So that's frustrating […] I notice that within the social sciences. […] I don't care what it's called. [It's] so trying, to get through with a common language.
>
> Health Equity Researcher (Epidemiology PhD)

Viewed in the light of the results of this thesis (demonstrating the connection between methods, theory and epistemology) it is perhaps unsurprising that researchers cling tightly to their preferred terms, and the terms of their own discipline, in interdisciplinary spaces. In previous results chapters, interviewees were particularly strident in defence of or advocation for analytical strategies leading to encounters with core, paradigmatic phenomena and concepts. This seems to extend into interdisciplinary encounters.

The interviewee quoted above referred to 'jargon' and this term merits further analysis. As a label, jargon might refer descriptively to terminology understandable by a particular group, as a part of what Bernstein (1971) termed a 'restricted code' (see literature review Section 4.2). Labelling a term as jargon may also have normative overtones, suggesting a term is unnecessary or does not convey important, specific meaning. Five interviewees mentioned jargon in the context of interdisciplinary work, and, exclusively, this referred to terminology from another discipline (or a different disciplinary specialisation) felt to convey no important meaning. One interviewee reflected on what they perceived as a proliferation of terminology throughout the span of their career:

> Qualitative stuff, people are getting too serious about it so they have come up with all this jargon. Are you doing this type of analysis or that type of analysis? Are you using this conceptual framework?
>
> When I read these things, [if] you cut all that crap out they're all doing the same thing! […] But if you don't use the magic words right now you get dinged for it. All of us have our 'magic words'. Anthropologists, statisticians have their magic words.
>
> Occupational Health researcher (Anthropology PhD)

This reference to certain terms as being magic (mandatory for recognition and acceptance) connects with broader views of language as performative within academia. Since 'questions of authenticity and legitimacy are central to the formation of social relations within the academy' (Archer, 2008 : p.389), the use of particular terms in research can signal familiarity with (and position new work in relation to) previous work, and/or demonstrate authors' normative values. However, these signals do not appear to translate across the boundaries of disciplines and specialties, appearing to others as 'jargon' and unwillingness to abandon them is perceived as 'picky' or 'precious'.

Rarer than these challenge of mismatch between terms and meanings was the situation in panel C of Figure 19, whereby terms or ideas simply cannot be translated across disciplinary boundaries. Three social scientists discussed situations where their efforts to communicate with members of other disciplines had broken down completely, because the other participant(s) did not appear to be able to engage with their approach:

> I've had early career experiences where I've been presenting stuff and the understanding is, the way they're viewing things is so different, that it's not there has been negative feedback. It just feels like you're talking past each other.
>
> TC: So, there's just no feedback?
>
> P: Yeah, no feedback. Or people are really perplexed, which doesn't then feel like a productive space.
>
> Professor of Public Health Policy (Geography PhD)

Such experiences may be relatively infrequently reported in my data because interviewees strategically avoid situations where they are unlikely to be understood. If productive academic exchange depends on researchers being able to communicate, it seems sensible that researchers avoid projects or environments where ideas or concepts central to their research are unfamiliar. But, even when concepts are shared, this analysis illustrates the way in which specialised terms for common concepts, or diverse meanings for common terms may make communication difficult, and overcoming these difficulties takes time, and effort:

> Extra time needs to be invested in developing the project as it goes along […] there are extra resources required to make the connections work.
>
> Geographer (Geography PhD)

In the context of an academy increasingly squeezed for time (Smith, 2010) and, in some contexts, driven to maximise output (Archer, 2008), this extra work may cause some researchers to avoid such projects altogether, or to collaborate only with near-neighbours, who speak similar languages:

> I haven't worked with that many sociologists, now that I think about it. It is

> mostly social psychologists and health psychologists. I feel like we're often speaking a similar language. I have not collaborated with economists, but when I hear them talk about things I am very confused, so I don't end up working with them as much.
>
> Social Epidemiologist A (Epidemiology PhD)

While many researchers spoke of desiring a common language, no one claimed to have arrived at one. Perhaps because of this, many interviewees highlighted 'needing to speak their language' in collaboration, or the importance of efforts to understand other disciplinary languages, which some researchers had learned to do:

> It's important to be able to reach across the aisle to the basic scientists, and talk a bit in their language. I feel the same way about my statistician colleagues. I am going to do the best I can to make things easy for them.
>
> Health Behaviour Researcher (Health Behaviour PhD)

> The problem is, of course, it is like different tribes. You need to be able to communicate with the other people, in their own language. You need to be able to understand what they are saying.
>
> Health Equity Researcher (Sociology PhD)

However, such widespread enduring difficulty with communication suggests the ability to speak multiple disciplinary languages not typical. If a common language is a mirage, or too time-consuming to hammer out in modern academic contexts, this opens up opportunities for researchers who are able to function as the glue holding a project together, linguistically. Six interviewees described themselves in these terms ('you're the glue that connects them all'), as synthesisers of diverse disciplinary approaches in service of their scientific questions. These researchers were all at professorial level, had all published more than 30 articles meeting the criteria for inclusion in my bibliometric analysis, and two of the six are among the top 5% of network members, in terms of in-network citations. Five of these six specifically mentioned the importance of 'doing your homework' prior to interdisciplinary collaboration, and stressed the value of their sustained efforts to read outside their discipline.

> I get to grapple with a lot of questions which enable me and require me, in my way, to read in history, biology, philosophy of science, science studies, and all kinds of social sciences.
>
> Social Epidemiologist B (Epidemiology PhD)

Although, not all of these interviewees practiced a collaborative form of interdisciplinarity:

> The 'reaching out' I do is primarily reading […] The interdisciplinary stuff comes from me reading other stuff, and being able to make sense of it. […] I wouldn't get in touch with them and say "do you want to collaborate on something?" I don't work that way.
>
> Health Policy Researcher (Psychology PhD)

This contrasted with the strategies of two interviewees, earlier in their careers, who confessed (somewhat sheepishly) that they struggled to read actively even within their own discipline, as the pressures of grant writing and publication targets felt all-consuming:

> I honestly, I am probably not reading as much work as I should. […] To fully confess, I focus on my discipline in terms of what I read. I try and read outside…. Well… I don't really try to read outside (laughing). I have a hard time keeping up.
>
> Social Epidemiologist A (Epidemiology PhD)

The prospect of investing time in reading and understanding a new literature did not seem possible for this interviewee. However, they had cultivated a reputation in their department as a valuable interdisciplinary collaborator, in a revealing demonstration of how interdisciplinary research can play out in varied ways for researchers at different career stages, and how interdisciplinary box-ticking might benefit researchers at the 'periphery', depending upon institutional context:

> Here in the medical school it is interesting, because for them I am the social scientist. So everyone is like "I need you on this grant" […] I am a social epidemiologist but it counts, here it counts [as social science]. […] For a lot of people I am what makes their work interdisciplinary, even though I know it's not quite true.
>
> TC: You don't think of yourself as a 'real' social scientist?

> Not really, no. Just because of my training [in social epidemiology] I think I have an understanding. I don't know what a sociologist would call me, I guess they would consider me an epidemiologist. I would be interested to hear what others, who I think are 'real' social scientists, would think of me. But I feel like I am not the same [as them]. […]
>
> It works because I can convince the clinical people. I am in a situation where I need to write a lot of grants, so for me that works, if people consider me that [as a social scientist]. I probably need to do better at collaborating with people who *are* social scientists. But, it works well enough for the grantsmanship side. So I think, ok, I will play along, and be the 'social scientist'.
>
> TC: How do those collaborations work out?
>
> They work out well, I speak that language pretty well.
>
> Social Epidemiologist B (Epidemiology PhD)

The above extract reveals how the benefits and challenges discussed in this chapter might intersect, in practice. I will mention and briefly discuss three features: First, this researcher does not view themselves as a social scientist, but for clinical collaborators looking for a team member who can tick the social-science 'box', they fit the bill. In precisely the way Abbott (2001) describes, what appears to matter is not an individual's position in the grand (cultural) social science / natural science distinction, but the *local* distinction, the extent to which a researcher is "social" relative to what is usual in a particular research context.

Second, the pressure on this researcher to win competitive grant funding is clearly shaping their willingness to 'play along' and 'play the social scientist' on such projects. As was mentioned in the introduction of this chapter, very few interviewees described feeling able to pursue a purely question-driven research agenda.

Third, this interviewee attributes their success to an ability to command a non-epidemiological disciplinary lexicon, *not* to an ability to stabilise an enriched, interdisciplinary creole within a collaborative setting. Most interviewees discussing interdisciplinary work expressed a desire for a common language with which to communicate. But, in discussing and describing interdisciplinary successes, interviewees overwhelmingly discussed (or described themselves as) *bilingual individuals*, as Kuhn (1989) suggested was necessary for meaningful communication across paradigms.

However, when evaluated against mono-disciplinary experts in their 'home' disciplines, these bilingual researchers (seemingly key to the success of interdisciplinary science) may find that their skill in speaking two disciplinary languages is not formally valued:

> Sometimes it takes a lot of time and then nothing comes out of it. It becomes more of "well, we learnt to know each other" […] But when you hand in applications to research councils, you're not really getting credit for that kind of effort. […] It may be said in different documents that cross disciplinary research is important and blah, blah, but often you are [then] evaluated against people who are more [mono-disciplinary] experts.
>
> Health Equity Researcher (Sociology PhD)

This may perhaps help to explain interviewees' sense that 'true' interdisciplinarity (which fully integrates disciplinary perspectives) is rare. In the context of the challenges discussed in this chapter, a picture emerges of the kind of project most likely to 'deliver': a collaboration between closely-related disciplines sharing similar or overlapping lexica, with project-elements which are easy to decompose into mono-disciplinary outputs likely to be successfully published in high-impact journals. Nine interviewees described interdisciplinary projects with which they had been involved as 'fragmented':

> People love talking about multidisciplinary teams and multidisciplinary centres, but my experience is that they tend to be very fragmented, along disciplinary lines.
>
> Health Economist B (Economics PhD)

This sense of interdisciplinarity as tending to be narrow, or local, reflects findings from bibliometric studies of interdisciplinarity over several decades. Porter & Chubin (1985) reviewed citation patterns in 383 articles drawn from 19 diverse journals. Distant citations (from or to engineering, life sciences, physical sciences, and social sciences) were extremely infrequent and the median number of such citations within papers was zero. In a later study encompassing 30 years of data, Porter and Rafols (2009) concluded that the distribution of citations has remained mainly within neighbouring disciplines. When compared with citation patterns from the 1970's and 1980's, 21st century science is becoming more interdisciplinary, but only by degrees, drawing primarily from neighbouring fields and only modestly increasing connectivity between distant areas. Interviewees noticed that collaboration seemed most common between 'neighbouring' disciplines.

> I think when people think about interdisciplinary work they might be thinking "we will get an epidemiologist together with a microbiologist" and they're all actually closely connected to clinical medicine. Which is great, they do fantastic work. But […] I think that sometimes the definition of interdisciplinary [work] is actually quite narrow. […] They are finding it a bit more difficult to step over the line towards the social sciences or arts and humanities.
>
> Geographer (Geography PhD)

This analysis has shown specific reasons why scholars might be reluctant to 'step over the line'.

10.6 Conclusion

This chapter discussed the balance between research questions and research methods, and presented the benefits and challenges of interdisciplinary HIDR.

The vast majority of interviewees reported that they value and enjoy interdisciplinary collaboration. Personal benefits included being exposed to new perspectives, and the rewarding, productive challenge of understanding and integrating those perspectives. Scientific or intellectual benefits included accessing new questions, new methods, the co-production of knowledge and access to the power of other research groups.

These widely-perceived benefits were tempered by the significant, practical challenges of working across disciplines. Difficulty publishing the output of interdisciplinary projects intersects with career-stage dynamics and a wider sense that, in general, hyper-specialisation represents the surest route to a secure academic career.

Interviewees discussed ’confidence’ and ‘credibility’ as pre-requisites for venturing across disciplinary divides, however close analysis of these comments suggested an underlying view of interdisciplinary spaces as potentially damaging or demoralising for those lacking the wherewithal to endure disciplinary prejudice (Lamont, 2009). Such risks can be got around in teams which respect each other, and are focused squarely on the research question, however this seemed to be achievable only in rare cases, after long time-periods of ‘trial and error’ searching for the right collaborators.

In practice, getting along or ‘clicking’ with collaborators seemed to reflect alignment in a number of areas. Researchers may not get along with a collaborator who minimises or disregards their discipline (expressed in a sense that some disciplines are relegated to the ‘periphery’, and are not at the ‘core’ of collaborative effort). Researchers also cannot get along with a collaborator with whom they cannot communicate, and language emerged as the biggest challenge of interdisciplinarity, and a source of hard work, requiring researchers to both learn and teach. The same terms can have different meanings, the same meanings can have different terms, or communication may fail entirely. Relating to language, ‘getting along’ involved a flexibility with regard to terminology, and a willingness to employ the terms of other disciplines - difficult when disciplines each have their own ‘magic words’.

The Kuhnian theoretical lens helped to tease out the ways comparison and evaluation of scientific statements depends upon scientific lexica, and Kuhn’s assertion that true interdisciplinarity requires bilingual scholars (rather than ‘enriched lexica’) was supported by interview data. Some interviewees had built careers on their ability to familiarise themselves with and integrate multiple disciplinary languages, however, the time required to develop this capability did not appear to be available to all interviewees, and, even where communication succeeded, this was not necessarily viewed (itself) as success within institutional evaluations, or competitive grant panels:

> It is a huge investment that each individual researcher has to make, but it is also an investment that the whole research society has to make. If you want cross-disciplinary research, you need to put a value on people knowing more languages than one. […] if the only thing that is valued by the system is that you dig deeper in your own discipline, there is going to be very little cross-disciplinary research, in the end.
>
> Health Equity Researcher (Sociology PhD)

In light of so many intersecting challenges it may be surprising that the majority of interviewees continue to attempt collaborative research. This persistence is partially explained by strategic manoeuvres (e.g., the social epidemiologist 'playing along' with clinical collaborators). But, in the majority of cases, continued commitment to collaboration seemed to be sustained by a powerful commitment to question-driven science, and a sense that the central questions of health equity cannot be satisfactorily answered via approaches from a single discipline.

Chapter 11: Concluding Discussion

11.1 Introduction

The aims of this thesis were threefold, to:

> 1: Identify the blend of formal disciplinary training present among health equity researchers, enabling analysis of the distribution of disciplines across the research area.
>
> 2: Explore diversity in epistemological approaches to health equity research, including considering how such diversity corresponds with disciplinary background, manifests in the design and evaluation of research studies, and intersects with other factors.
>
> 3: Assess the impact of disciplinary differences on interdisciplinary and collaborative research efforts within HIDR.

The first aim was approached via bibliometric analysis, reported in Chapter 6. The second and third aims were approached via qualitative methods, reported in Chapters 7-9 and 10, respectively.

This concluding chapter begins with a summary and discussion of the key findings. The practical implications of my findings for HIDR (and members of the disciplines discussed in the thesis) are discussed in Section 11.3. The thesis then concludes with a discussion of the thesis's contribution to theoretical and conceptual treatments of academic disciplines, in general and in health-related research.

11.2 Summarising results

This PhD thesis has been an in-depth investigation of the ways academic disciplines shape scientific practice in health equity research. Whilst covering many contexts and aspects of research practice, the thesis has focused on the way disciplinary training shapes knowledge construction, and the tension this produces in multi-disciplinary research domains.

11.2.1 An Atlas of Health Inequalities and Disparities Research

In Chapter 6, a bibliometric network was presented which included the

250 most-connected health equity researchers. Health equity scholars represent a large number of disciplinary backgrounds, but this diversity is not distributed evenly across the field. Detailed analysis of the network's eight clusters revealed the presence of disciplinary silos, and a historical review of HIDR in the US and UK brought further context and nuance to the interpretation of the network's configuration. Although disciplinary training appeared to play a role in the emergence of the research clusters, so too did historical, geographic, institutional and financial (research funding) forces. This visual representation of the field helped unpack divergent views expressed by interviewees in later chapters, and, in turn, interview data helped to elaborate new interpretations of the network (e.g., interviewees' discussions of the field in terms of 'periphery' and 'core' presented in Section 10.4.2).

11.2.2 Ways of Knowing

In Chapters 7 and 8, I drew upon interview data to demonstrate that there is no unified epistemology connecting HIDR. Disciplinary training seems to powerfully and enduringly shapes the choices which feel 'natural' or 'normal' for scientists, with multiple kinds of 'normal' coexisting within the multidisciplinary domain of health equity research. Strategies for knowing and kinds of knowledge mapped predictably (though, imperfectly) onto interviewees' disciplinary training, and onto regions of the bibliometric network. These differences illustrated variation between disciplines in tolerance for complexity, strong and weak disciplinary classification, and integrative versus cumulative codes, and also signalled the presence of Lamont's (2009) four epistemological styles within HIDR (comprehensive, constructivist, positivist, & utilitarian), explored in the paragraphs below.

Some researchers appeared to conceptualise health equity as a phenomenon located within society (especially interviewees from network Clusters 7, 4 and 6). For these researchers, social structures enter the epistemology as research objects in their own right, worthy of empirical study. Formal training in (or other exposure to) social science appeared to be a precondition for recognition of social structures and processes as research objects. Social scientists tended to value holistic approaches and attention to context (Lamont's *comprehensive style*) as well as approaches which witness or give voice to lived experience (the *constructivist style*). The extent to which disciplinary paradigms can accommodate a comprehensive epistemological style, or require the decomposition of phenomena into parts emerged as particularly important (e.g., a clear difference was apparent between interviewees who value 'messy policy stuff' and those valuing analytical 'elegance').

Other interviewees approached health equity with a stronger focus on distribution of disease and pathophysiology. These interviewees were more likely to be located in network clusters 1, 3 and 8. Some of these interviewees viewed the study of social forces as generating a less-authoritative kind of evidence due to perceived distance from disease along temporal/causal pathways. These interviewees did not discard or reject the social determinants, but these tended to enter the epistemology as 'exposures' alongside, and in the same manner as behavioural or biological factors (to answer questions about disease distribution, rather than questions about society). These researchers were likely to have epidemiological or medical training, and to value concrete knowledge relating to solutions, which, in some contexts, meant randomised or pseudo-randomised studies of policy interventions. This blend of the *positivist* and *utilitarian styles* was represented by a small minority of Lamont's participants, drawn from the humanities and social sciences. Among the population health researchers interviewed in this study, the instrumental focus was blended with all other styles (i.e., interviewees of diverse disciplinary backgrounds expressed a preference for 'useful' knowledge). However, while a strong desire to obtain useful knowledge cut across disciplines, this was not a unifying epistemological feature as ideas about what is 'useful' demonstrably varied across disciplinary groups.

Complicating the above distinction are social epidemiologists (located mostly at the intersection of clusters 1, 2 and 3), working actively to bring the comprehensive and constructivist styles into epidemiological research. The manner and extent of epistemological integration seemed to vary among social epidemiologists interviewed. For example, some social epidemiologists engage deeply with sociological theory and/or utilise qualitative research methods, and others use multi-level modelling to add an extra (quantitative) layer to epidemiological studies. Social epidemiologists generally valued attention to contextual, contingent factors (a key virtue in the social sciences), but also identified rigorous design and avoidance of bias (key virtues in epidemiology) as hallmarks of good empirical research. This group are discussed further in Section 11.3.

A fourth set of interviewees based in the US (small in number but the subject of frequent, forceful critique) were behavioural scientists, who discussed aiming to improve health equity by studying and changing individual behaviour. These interviewees were located at the intersection of clusters 3 and 8. Knowledge about behaviour was valued and sought empirically because it can be harnessed in the development of resources and programmes designed to encourage the public to behave in ways which minimise disease risk. In Chapters 7 and 8, similarities emerged between this group and social scientists based in the UK and Europe, although the behavioural science paradigm reportedly constrained the way in which (and the extent to which) ideas about behaviour originating from constructivist or comprehensive epistemological styles could be expressed.

A final group, primarily located in Cluster 2, appeared to seek another form of knowledge: not social, biological or behavioural, but technical. Some economists, epidemiologists and biostatisticians study health primarily via pursuit of *negative knowledge,* knowledge about how to obtain knowledge (Knorr-Cetina, 1999). I am not aware of any previous study presenting the balance of negative and positive knowledges as a potentially important component of disciplinary difference, nor any study highlighting this distinction as important in research about population health.[22] Future research might explore this finding, by focusing on the ways negative and positive knowledges are blended, within institutions, within teams, and in individual research practice.

I have identified fewer groups than there are clusters in the bibliometric analysis, suggesting that it is useful to discuss epistemological “regions” of HIDR, rather than singling out particular clusters. One cluster (Cluster 5) was not represented among my interviewees, and so I can make no statement about the dominant epistemology of that cluster. Cluster 3 (the large, US-dominated cluster) did not seem to represent a distinct epistemology but rather a microcosm of the wider network, containing clinical, social, and behavioural scientists, distributed in a manner echoing the network’s wider structure (a core of epidemiologists, social being toward the left, clinical toward the right, and other disciplines at the periphery). The geography-dominated Cluster 7 may represent a distinct approach, but as I discuss further in Section 11.3.1, I have not been able to include detailed analyses of individual social-sciences (beyond sociology) in the thesis.

These diverse knowledges connect with previous studies of HIDR introduced in Chapter 1 (Section 1.5). Garthwaite and colleagues (2016) identified three 'ideal-types' of health inequalities researcher. This PhD project has identified more than three 'types', perhaps unsurprising as this study was much broader in methodological, disciplinary and geographic terms. Smith & Eltanani (2014) reported reasonable consensus among their sample that upstream interventions are likely to have the most significant impact on health inequalities, but, when participants were asked to rank interventions on the strength of the available evidence, responses were less cohesive. Findings in this thesis provide multiple potential explanations for this finding. First, the preference for instrumental knowledge in epidemiology

[22] A search of PubMed (13/11/20) for the term "negative knowledge" returned 18 results, none of which refer to this theoretical concept, mostly presenting examples of sentences ending with the word 'negative' followed by sentences starting with the word 'knowledge': "negative. Knowledge…".

(coupled with the inherent challenges of randomising participants into social circumstances) complicates the generation of evidence broadly recognised as 'strong'. This (perceived) barrier to studying the structural determinants intersects with the generative power of epidemiology within HIDR, and the importance of demonstrating 'epidemiological rigour' (which in turn seemed to shape views regarding what is fundable, and publishable), potentially incentivising small trials of interventions over large observational studies of structural determinants.

In Chapter 8, interviewees' sense of what is 'worth doing' revealed the way paradigmatic concepts and concerns can shape methodological preference. In their study of UK-based health equity researchers, Garthwaite et al (2016:p.466) described 'Obvious methodological tension within the multi-disciplinary field of health inequalities'. This thesis has unpacked the foundation of such tensions, demonstrated that they cross geographic contexts, and provided clarity regarding why certain epistemologies may not be compatible with particular methods.

Findings in Chapter 8 also caution against the assumption that research methods are simple, interchangeable, tools. Rather, methods, knowledge, and the purpose of research are intimately connected, and the sets of methods favoured within a discipline can be expected to represent favoured strategies for encountering paradigmatic concepts. The decision to employ a method often appears to have three precursors: being familiar with the method, familiar with the phenomena the method is suited to interrogate, and having the belief that insight regarding that phenomena is of value. In Chapter 9 the status of regression coefficients as reflecting reality in a straightforward way was questioned by almost all interviewees. Similarly, the concept of 'statistical significance' was broadly viewed as lacking meaning, but no consensus was evident regarding what should replace it ('social'/'clinical'/'practical' significance). In Chapter 10, tension was apparent between research methods and research questions, which in some cases seemed to reflect a restrictive focus on method within the strongly-classified epidemiological and economic paradigms.

In addition to these epistemological differences, Chapters 7-10 documented concrete differences in research practice across disciplines, connected to and reflecting more abstract, epistemological differences. In Chapter 9, health economists discussed reporting multiple iterations of statistical models, and fully declaring the form of such models (in contrast to more sparse presentation in epidemiology and public health), reflecting the cardinal importance of statistical methodology and internal validity in health economics. Researchers from different disciplinary backgrounds employed qualitative methods for different reasons: social scientists tended to view this as a stand-alone method whereas epidemiologists and economists reported conducting qualitative research for the purpose of improving or optimising quantitative research. In multiple chapters, the form and length of publications were described as varying across disciplines, with implications for the kinds of knowledge which can be communicated and disseminated. The existence of diverse genres is itself a signal that diverse forms of knowledge are in play (Bazerman, 1988), and this study could be fruitfully extended via dedicated analysis of written texts from network regions, or clusters. In Chapter 10, research was described as proceeding at different paces in different disciplines, with clearly different norms surrounding publication co-authorship acting as barriers to interdisciplinary research.

11.3. Practical implications

Conditions facilitating ‘cognitive contextualisation’ (the evaluation of research products via standards belonging to the discipline in which they are produced; Lamont, 2009) appeared to be the exception, and not the rule in HIDR. This suggests that rather than being permitted to shine under the light of the discipline from which it originates, research tends to be examined and evaluated through the lens of each researcher’s own training and epistemological priorities. In this section, I explore the practical implications for researchers in the areas of focus in this thesis.

11.3.1 Social Scientists

The status of social science within HIDR appears slightly paradoxical: on the one hand, contextualist, constructivist approaches were very widely valued, and calls for increased attention to 'upstream' determinants has been a feature of the literature for many decades. However, the dominant (epidemiological, positivist) epistemology was presented by some social scientists as being a restrictive conduit for the kind of insight generated (and valued) within the social sciences. This manifested in various areas, including research funding, publication opportunities, and power-dynamics in collaboration. Variation *among* social scientists has been under-analysed in this thesis, primarily because such variation was overwhelmed by the differences between social scientists and other groups of interviewees. However, this variation is important to explore, in future studies.

11.3.2 Health Economics

Bartley (1992) positioned health economists as the "natural competitor" of the epidemiologist and biostatistician (p.176) in terms of policy influence. This dynamic was not evident in this research-focused study. Rather, health economists interviewed had their own clear picture of what 'accuracy' and 'objectivity' entail, were generally located in different academic departments (not co-located with epidemiologists and biostatisticians) with different evaluative cultures, and publishing in different journals. Most interviewees from other disciplines tended to view health economists as operating in a separate space, and to regard their activities with (at least) mild confusion. A minority had struck productive working relationships with economic collaborators, and found their technical capacity and command of the 'economic language' to be extremely valuable.

However, as Bartley's analysis (and other studies of economists, reviewed in Mäki, 2001) anticipates, health economists interviewed were more likely than other groups to favour individualist explanations and interventions for health inequity, and to question the causal connection between income and health, creating predictable tension with social scientists and social epidemiologists. In addition, the balance between negative and positive knowledge in health economics appeared distinctive. This preference for individual-level analysis and very strong technical focus echoes the technical tone of exchange between WHO economists and social epidemiologists referred to at the beginning of the thesis, suggesting that the underlying difference in approach which catalysed that debate in has persisted during the intervening decades.

11.3.3 EPIDEMIOLOGY

In setting out, I sought to identify and describe differences between disciplines. However, emergent in interview data was the importance of tension *within* epidemiology in shaping research. While epidemiologists were remarkably consistent in their views about ‘good empirical work’, and I concluded that the discipline is strongly classified on that basis, the stark differences between social and clinical epidemiologists interviewed merit consideration.

Pearce (1996) argued that there are in fact two epidemiological paradigms. 'Traditional' epidemiology places disease in a cultural and historical context, takes a structural view of causation (thereby favouring 'upstream' interventions), and incorporates elements from demography and the social sciences. ‘Traditional’ epidemiology as defined by Pearce therefore favours the comprehensive thought-style (Lamont, 2009). 'Modern' epidemiology, on the other hand, favours the positivist style, is focused on pathophysiology, aims to divorce disease from context, is centred on the clinical paradigm, and focused on individual-level interventions. Pearce noted that:

> Traditional epidemiology has become unfashionable and is treated somewhat disparagingly in modem epidemiology texts [...] there has been a strong focus on statistical issues [...] and an ignorance of the other modes of thought. (p.679)

Since 1996, what Pearce termed 'traditional' has emerged as a mature sub-specialisation: 'social' epidemiology (Honjo, 2004). The emergence of a structurally-focused specialism might be viewed as a triumph on one hand, a reflection of the importance of the social determinants of health. However, it is also possible (and suggested by my results) that this reflects what Pearce contended: the comprehensive thought-style is no longer a part of epidemiology's 'core', but is peripheral to modern epidemiology, for specialised scholars with particular interests.

In this thesis, social epidemiologists discussed struggling to gain recognition in 'mainstream' epidemiology, needing to 'make the case' for their approach and their methods, the difficulty of fitting their analyses into 3000-word journal articles, and involvement in 'box-ticking' interdisciplinary projects. Together, these findings hint at an uneasy balance between these epidemiologies (in population health), and suggest social epidemiology is not embraced in all settings, even if it is prominent within HIDR.

11.3.4 Cross-Cutting Factors: Publication, Funding & Promotion

Cutting across disciplines was the epistemic force of research funding. In Chapter 7, funding was presented as directly shaping research questions, with projects generating knowledge about specific diseases perceived as more fundable than knowledge about societal or commercial determinants. In Chapter 8, funding streams were described as influencing methodological choices, with certain methods being perceived as more or less fundable than others. In particular, qualitative researchers described needing to advocate for and explain their methods. The status of epidemiological journals as high-impact also served to shape the kinds of studies viewed as publishable, connecting with Walsham's (1995) view of scientific journals as disciplinary instruments, and description of peer review as a disciplinary examination. A connective thread emerges in these findings: generally speaking, that which adheres to the modern epidemiological approach is fundable, and publishable, whereas research focused on social or upstream determinants may be more challenging to (financially) sustain.

Smith (2013) concluded that "micro-political, career interests are crucial to understanding the interplay between public health research and policy" (p.10), as these help to explain how and why research claims about health inequalities are constructed as they are. This thesis has provided additional empirical detail on this point, and my findings align with Smith's conclusions: pursuit of secure funding streams appears to nudge researchers toward disease focused, individual-level, 'downstream' research. While not all interviewees reported their work being shaped in such ways, there is at least some evidence that in order to sustain the cycle of credit (Latour & Woolgar, 1986) some researchers are actively minimising the presence of comprehensive or constructivist thought-styles in their research proposals, and published works.

11.4 A recent interdisciplinary meeting

In May 2018, an international meeting of HIDR scholars was held in Amsterdam, hosted by the Royal Netherlands Academy of Arts and Science (Mackenbach & De Jong, 2018). This meeting provides a real-world test of some claims made in this concluding chapter, and an opportunity to test some key findings of the thesis.

The meeting focused on the connection between income and health, seeking to resolve:

> "uncertainty and controversy about the extent to which the relationship [between income and health] reflects **causation** (socioeconomic position influences health), **reverse causation** (health influences socioeconomic position) **or confounding** (by factors that affect both socioeconomic position and health)" (ibid., p.2, emphasis added)

Whilst I was aware of the meeting in 2018, I did not access the formal report until October 2020, as I wished to use this document as a way of checking whether key differences emerging from my analysis were apparent in a real-world interdisciplinary discussion.

Three interviewees briefly mentioned the meeting in their interviews. Social epidemiologists and social scientists in attendance (reflected in interview data, and the meeting's formal report) found the framing of the symposium's question, and the project discussion paper "a rather simplistic view" (ibid., p12) of the relationship between income and health. This aligns with my results suggesting that social scientists tend to value complexity and attention to contextual detail in analysis. The value of decomposing causal effects (e.g. isolating the impact of education on health from the impact of income on health) was specifically and actively contested, with social scientists and social epidemiologists on one hand questioning this decomposition, and economists and some epidemiologists advocating for the value of such decomposition. Discussion was summarised in the official report as reflecting the absence of "consensus on the relative importance of causation and reverse causation […] no consensus on the relative importance of confounding factors, nor on how confounding should be conceptualised" (p.5).

In addition, discussion revealed a lack of consensus regarding both whether participants viewed the question 'does low socioeconomic position cause ill health?' as *answerable* (p.18), and whether any answer could be understood as having *meaning*. Attendees viewed achieving resolution of these points as "highly relevant to policy development" (p.19). However, this thesis anticipates such conflict, and suggests that resolution of these points is quite unlikely. Desire to obtain atomised, decomposed causal effects is not simple preference, but a reflection of an interconnected set of epistemological commitments, originating within the motivating puzzles of clinical epidemiology and mainstream economics. Similarly, commitment to the 'interconnectedness of things' appears to be grounded within the epistemic fabric of the social sciences, not something to be discarded on the way to well-identified causal estimates.

A lack of consensus after lengthy debate and discussion (including debate about whether a question is ‘answerable’, or whether an answer has any ‘meaning’) calls forward the Kuhnian view of disciplinary cross-talk, presented in Chapter 10. It does seem to be the case that, as Kuhn asserted, there is no such thing as being simply right or wrong in such discussions, as there are multiple 'rights' and 'wrongs' available, and no neutral territory from which to choose between them. Kuhn's originating contribution to STS (presented in Chapter 2) was his view of scientific theory as shaping both what is observed, and how observations are interpreted. Kuhn's position – that even the most apparently straightforward scientific statements are embedded in theoretical systems – appears to remain relevant, and to help explain why some debates do not resolve within HIDR (and population health), even over multiple decades.

In the next section I explore the theoretical and conceptual contribution of the thesis more fully.

11.5 Theoretical & Conceptual Contributions

As was outlined in the literature review, various independent strands of research about disciplines are available, with much re-inventing of the (conceptual) wheel among them. In presenting a coherent analytical strategy for addressing disciplines, and highlighting particular concepts as analytically fruitful, this thesis lays the groundwork for future studies of academic disciplines in health-related fields, and beyond. Whilst I have dodged the difficult question 'what is a discipline?', I have taken a step toward answering what I view as the more important question: 'what matters about disciplines, in research about population health, and how does disciplinary difference tend to manifest?' This section reviews the conceptual contributions I feel are most important to carry forward into future studies.

11.4.1 Classification

Disciplinary classification, the relative strength or weakness of disciplinary boundaries, categories and identities, has independently emerged by various names in diverse literatures (Biglan, 1973; Kolb, 1981;

Becher & Trowler, 2001; Abbott, 2001; Lamont, 2009). This concept featured in this thesis as a particularly important dimension of disciplinary difference, and appears to represent a key, enduring feature of academic disciplines. Becher & Trowler (2001) conclude that academics are most likely to be empowered (professionally) within strongly-classified, cumulative disciplines, suggesting members of certain disciplines may enter interdisciplinary spaces on an unequal footing. This was indeed reported in Chapter 10, with medical and epidemiological collaborators reportedly enjoying shortcuts to credibility and capacity to enrol members of other disciplines in projects without the intention of epistemological compromise (described as 'box-ticking'). However, economists also reported being enrolled in 'box-ticking' projects, perhaps reflecting the special status and epistemic force of medicine and epidemiology within public and population health. Viewed another way, this finding may reflect the (relative) flexibility of the social sciences: a lack of epistemic rigidity may enable researchers with social science training to more easily work with others, and within interdisciplinary teams.

Readers with social science backgrounds may reject my conclusion that epidemiology is strongly classified and the social sciences are (or, tend to be) weakly classified. However, 'classification' is *relative* (Abbott, 2001), and interview data do support the conclusion that epidemiology has a more consistent set of questions and methods, reflected in relatively short article lengths. Local distinctions (e.g., within the social sciences of a single academic School) may be more important in the working lives of researchers than the global distinctions drawn in this thesis (e.g., between epidemiology and sociology). Disciplines certainly exhibit local and regional variation (Ruscio, 1987); however, the capacity for editors of epidemiology journals to powerfully shape the field across disciplinary and geographic boundaries presents one example where, perhaps, such global distinctions are important, and impactful.

11.4.2 Cognitive Contextualisation

The interdependent disciplinary differences presented in this thesis signal the presence of distinct epistemic cultures, clearest in interviewees' discussions of what felt 'usual', 'normal' or 'common-sense', and in discussions of the hallmarks of 'good' research. Collectively, these findings lend empirical support and detail to general claims such as Neumann & Becher's (2002: p.410) that disciplines differ in their "main cognitive purposes", or Sa's (2008: p.549) reference to the "deep roots of traditional academic structures". Similarly, Lamont (2009) concluded that disciplinary variability in criteria for academic excellence was rooted in the way disciplines' "objects and concerns differ so dramatically" (2009:p.9). In this thesis, I have connected such observed difference to *specific* 'objects and concerns', where data permitted (chiefly within epidemiology, economics and sociology).

Evidence of cognitive contextualisation was limited, though not entirely absent (see Chapter 7 Section 2.1.5). Data presented in Chapter 8 (p.219) demonstrated a tendency for the critique of disciplines on the basis of what they neglect or do not contain, sometimes with little accommodation or regard for the aims and priorities of those disciplines. It seems that, without the intervention of a skilled panel chair (as was available in the grant review panels studied by Lamont), researchers struggle to overcome their 'disciplinary prejudice', and to allow work from other traditions to shine under its own lights.

11.4.3 The Kuhnian Lens

Throughout the thesis, theory from STS and SSK (and adherence to the causal tenet of Strong Programme SSK) helped to go beyond describing apparent differences between disciplines, and opened inquiry to consideration surrounding underlying epistemological styles and cultures. In turn, exploring the epistemological drivers of disciplinary difference has enabled some tentative observations about the nature of disciplines in health-related research.

In particular, the application of Kuhnian theory helped to move beyond description, toward explanation, and to draw out the kinds of epistemological commitments embodied in scientific practice. Very generally, Kuhn's description of disciplinary paradigms as patterns of investigative practice which endure throughout a career was apparent in results. Disciplinary training clearly informed the 'mental model' of many interviewees, the kinds of questions they considered interesting, and the forms answers to those questions are expected to take.

Three specific Kuhnian insights were particularly important in this thesis:

1. Understanding the paradigm as the 'backdrop' which throws certain practices into relief, and obscures others as 'natural', 'normal' or 'common-sense.'

2. Understanding research methods as special tools which serve special functions, and the difficulty of discussing or evaluating methods in isolation from the insight they generate.

3. Understanding disciplinary lexica as potentially inhibiting interdisciplinary communication and collaboration, highlighting the importance of researchers who speak multiple disciplinary languages.

In Chapter 2, I noted that disciplines are knowledge-communities first and foremost, but are also the site of varied social functions (writing, publishing, teaching, learning). This plurality complicates the study of disciplines. The above set of Kuhnian concepts is reflective of this plural character, and the disciplinary matrix is a theoretical frame especially well-suited to addressing disciplines in all their varied manifestations – as a set of separate-but-connected activities conceptualised as the set of elements within a matrix.

My empirical material leads me to think there is a strong case for a rehabilitation of the Kuhnian view of disciplines. Kuhn's writings and sense of disciplinary paradigms has been key to recognising and describing the mechanism of disciplinary influence in this thesis, and might be applied fruitfully in other multidisciplinary research domains.

11.6 Conclusion

This chapter has synthesised the findings explored throughout the thesis, and offered a reflection on the central theoretical insights about disciplines to emerge from results. The thesis concludes here with a consideration of strengths, limitations, and opportunities for future research.

First, a key strength is the inclusion of participants from a range of countries and institutions, reinforcing the value of a comparative & international sociology of science. This study has been able to go beyond previous single-country studies, to highlight the parts of disciplinary culture which might be expected to cut across geographic and institutional settings, and the way these aspects of disciplines shape research in population health. However, this breadth is also a weakness: My analysis has leaned toward the macro (continents/clusters) and the micro (interviewees), with much work still left to do in the meso, at the level of academic institutions, departments, and research teams.

Additionally, due to the size of this study and the limits of a PhD timeframe I was only able to consider epidemiologists, economists and sociology/'social scientists' in detail. Careful analysis of other disciplines (such as geography, anthropology, medicine and political science) would doubtlessly yield interesting and important insights. Further analysis of epidemiology, economics and sociology is also warranted, as close analysis of any discipline should be anticipated to reveal significant heterogeneity, and reflections of broader cultural distinctions (Abbott, 2001).

In addition to the micro-political interests discussed in Section 11.3, Smith's (2008; 2013) data suggested that political ideology was an important driver of research conduct, including successful interdisciplinary collaboration. As was reported in Chapter 6, the study of health inequalities was politicised at an early stage in the UK (where Smith's studies were conducted) and this may help to explain the importance of political views and ideology for researchers in the UK. However, beyond very general statements about academics tending to lean to the political left, no interviewee mentioned politics as an important factor in shaping research practice in this study. It may be that my strong focus on disciplines has caused other factors, such as politics and values, to be neglected in the thesis (and in interviews). The intersection of disciplinary difference with these factors will be important to clarify in future research.

Despite these limitations, a sustained focus on disciplines has yielded findings with significant implications for the practice and progress of research about health equity. Therefore, whilst I cannot yet conclude firmly that researchers under different paradigms have different "ways of seeing the world and practicing science in it" (Kuhn, 1970: p.4), I do conclude that health inequalities and disparities research is a scholarly community with diverse disciplinary backgrounds, working within different epistemic cultures, exploring diverse 'big-pictures', seeking diverse knowledges, via diverse methods. Despite critiques describing the notion of paradigms as being outdated, or monolithic (Biagioli, 1999; Galison, 2016; Brew & Manathunga, 2012), my data show that as well as being social entities, disciplines are cognitive enterprises which shape research in varied, important ways.

REFERENCES

Abbott, A. (1988). The System of Professions. University of Chicago Press, Chicago, United States.

Abbott, A. (2001). Chaos of Disciplines. University of Chicago Press, Chicago, United States.

Adams, T. L. (2015). Sociology of professions: International divergences and research directions. Work, Employment and Society, 29(1), 154–165. https://doi.org/10.1177/0950017014523467

Almeida-Filho, N., Kawachi, I., Filho, A. P., & Dachs, J. N. W. (2003). Research on Health Inequalities in Latin America and the Caribbean: Bibliometric Analysis (1971–2000) and Descriptive Content Analysis (1971–1995). American Journal of Public Health, 93(12), 2037–2043. https://doi.org/10.2105/AJPH.93.12.2037

AMA CEDA (Council on Ethical and Judicial Affairs) (1990). Black-white disparities in health care. JAMA, 263(17), 2344–2346. https://doi.org/10.1001/jama.1990.03440170066038

Amrhein, V., Greenland, S., & McShane, B. (2019). Scientists rise up against statistical significance. Nature, 567(7748), 305–307. https://doi.org/10.1038/d41586-019-00857-9

Ankeny, R., Chang, H., Boumans, M., & Boon, M. (2011). Introduction: Philosophy of science in practice. European Journal for Philosophy of Science, 1(3), 303. https://doi.org/10.1007/s13194-011-0036-4

Archer, L. (2008). Younger academics' constructions of "authenticity", "success" and professional identity. Studies in Higher Education 33, 385–403. https://doi.org/10.1080/03075070802211729

Armstrong, S.A., Henson, R.K. (2005). Statistical Practices of IJPT Researchers: A Review from 1993-2000. International Journal of Play Therapy 14, 7–26. https://doi.org/10.1037/h0088894

Ashwin, P. (2009). Analysing Teaching-Learning Interactions in Higher Education: Accounting for Structure and Agency. Bloomsbury Publishing.

Badenes-Ribera, L., Frias-Navarro, D., Pascual-Soler, M., Monterde-I-Bort, H. (2016). Knowledge level of effect size statistics, confidence intervals and meta-analysis in Spanish academic psychologists. Psicothema 28, 448–456. https://doi.org/10.7334/psicothema2016.24

Bagozzi, R. P. (2007). The legacy of the technology acceptance model and a proposal for a paradigm shift. Journal of the Association for Information Systems, 8(4), 3. https://doi.org/10.12691/ajis-3-2-3

Bambra, C., Smith, K. E., Garthwaite, K., Joyce, K. E., & Hunter, D. J. (2011). A labour of Sisyphus? Public policy and health inequalities research from the Black and Acheson Reports to the Marmot Review. Journal of Epidemiology & Community Health, 65(5), 399–406. https://doi.org/10.1136/jech.2010.111195

Bammer, G. (2013). Disciplining Interdisciplinarity. ANU Press. https://doi.org/10.22459/DI.01.2013

Bardet, F. (2008). When car manufacturers shape road safety mediatization of road accidents in the USA [Quand les constructeurs automobiles façonnent la securité routière: La médiatisation des accidents de la route aux états-unis]. Reseaux 147, 87–113. https://doi.org/10.3166/Reseaux.147.87-113

Bargh, J. A., McKenna, K. Y. A., & Fitzsimons, G. M. (2002). Can You See the Real Me? Activation and Expression of the “True Self” on the Internet. Journal of Social Issues, 58(1), 33–48. https://doi.org/10.1111/1540-4560.00247

Barker, D. J., & Osmond, C. (1986). Infant mortality, childhood nutrition, and ischaemic heart disease in England and Wales. Lancet (London, England), 1(8489), 1077–1081.

Barnes, B. (1982). T.S. Kuhn and social science. Columbia University Press.

Barnes, L. L. B., Agago, M. O., & Coombs, W. T. (1998). Effects of job-related stress on faculty intention to leave academia. Research in Higher Education, 39(4), 457–469. https://doi.org/10.1023/A:1018741404199

Bartley, M. (1990). Do we need a strong programme in medical sociology? Sociology of Health & Illness, 12(4), 371–390. https://doi.org/10.1111/j.1467-9566.1990.tb00078.x

Bartley, M. (1992). Authorities and partisans: The debate on unemployment and health. Columbia University Press.

Bastian, M., Heymann, S., & Jacomy, M. (2009). Gephi: An open source software for exploring and manipulating networks. Icwsm,8, 361–362.

Batagelj, V., & Mrvar, A. (2004). Pajek — Analysis and Visualization of Large Networks. In Graph Drawing Software (pp. 77–103). Springer, Berlin, Heidelberg. https://doi.org/10.1007/978-3-642-18638-7_4

Baum, F. (1995). Researching public health: Behind the qualitative-quantitative methodological debate. Social Science & Medicine, 40(4), 459–468. https://doi.org/10.1016/0277-9536(94)E0103-Y

Bazerman, C. (1988). Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science. University of Wisconsin Press.

Becher, T. (1987). The Disciplinary Shaping of the Profession. In B. R. Clark (Ed.), The Academic Profession. National, Disciplinary & Institutional Settings (pp. 271–303). Berkeley: University of California Press.

Becher, T. (1994). The significance of disciplinary differences. Studies in Higher Education, 19(2), 151–161. https://doi.org/10.1080/03075079412331382007

Becher, T., Trowler, P. (2001). Academic Tribes And Territories: Intellectual Enquiry and the Culture of Disciplines. 2nd Ed. McGraw-Hill Education (UK).

Benton, T., & Craib, I. (2010). Philosophy of Social Science: The Philosophical Foundations of Social Thought. Palgrave Macmillan.

Berman, E.P., Hirschman, D. (2018). The Sociology of Quantification: Where Are We Now? Contemp Sociol 47, 257–266. https://doi.org/10.1177/0094306118767649

Bernstein, B. (1971). Towards a Theory of Educational Transmissions. Routledge.

Bernstein, B. (1996). Pedagogy, symbolic control and identity. Theory, research, critique. London: Taylor & Francis.

Biagioli, M. (2009). Postdisciplinary Liaisons: Science Studies and the Humanities. Critical Inquiry, 35(4), 816–833. https://doi.org/10.1086/599586

Biglan, A. (1973). The characteristics of subject matter in different academic areas. Journal of Applied Psychology 57, 195–203. https://doi.org/10.1037/h0034701

Black, D., Morris, J. N., et al. (1980). Inequalities in health: report of a research working group. London: Department of Health and Social Services.

Bland, J.M., Altman, D.G. (2003). Applying the right statistics: Analyses of measurement studies. Ultrasound in Obstetrics & Gynecology 22, 85–93. https://doi.org/10.1002/uog.122

Blondel, V. D., Guillaume, J.-L., Lambiotte, R., & Lefebvre, E. (2008). Fast unfolding of communities in large networks. Journal of Statistical Mechanics: Theory and Experiment, 2008(10), P10008. https://doi.org/10.1088/1742-5468/2008/10/P10008

Bloor, D. (1976). Knowledge and Social Imagery (1st ed.). Chicago University Press.

Bloor, D. (2011). The Enigma of the Aerofoil. Chicago University Press.

Bouchard, L., Albertini, M., Batista, R., & de Montigny, J. (2015). Research on health inequalities: A bibliometric analysis (1966–2014). Social Science & Medicine, 141, 100–108. https://doi.org/10.1016/j.socscimed.2015.07.022

Boyack, K. W., & Klavans, R. (2010). Co-citation analysis, bibliographic coupling, and direct citation: Which citation approach represents the research front most accurately? Journal of the American Society for Information Science and Technology, 61(12), 2389–2404. https://doi.org/10.1002/asi.21419

Braveman, P. A., Cubbin, C., Egerter, S., Williams, D. R., & Pamuk, E. (2010). Socioeconomic Disparities in Health in the United States: What the Patterns Tell Us. American Journal of Public Health, 100(Suppl 1), S186–S196. https://doi.org/10.2105/AJPH.2009.166082

Braveman, P., Krieger, N., & Lynch, J. (2000). Health inequalities and social inequalities in health. Bulletin of the World Health Organization, 78(2), 232–234; discussion 234–235. https://doi.org/10.1177/1757975909103762

Brew, A., & Manathunga, C. (2012). Beyond tribes and territories. New metaphors for new times. In P. Trowler, M. Saunders, & V. Bamber (Eds.), Tribes and territories in the 21st century: Rethinking the significance of disciplines in higher education. Routledge. https://doi.org/10.4324/9780203136935

Bromberger, S. (1992). On What We Know We Don't Know: Explanation, Theory, Linguistics, and How Questions Shape Them. University of Chicago Press.

Bucher, R., & Strauss, A. (1961). Professions in process. American Journal of Sociology, 66(4), 325–334.

Camargo, A. de P. R. (2009). The sociology of statistics: The possibilities of a new field of investigation. História, Ciências, Saúde-Manguinhos, 16(4), 903–925. https://doi.org/10.1590/S0104-59702009000400004

Camargo, A. De P.R. (2015). The dimensions of the nation: An analysis of the statistical discourse of the general direction of statistics (1872-1930) [Dimensões da nação: Uma análise do discurso estat ístico da diretoria geral de estatística (1872-1930)]. Revista Brasileira de Ciencias Sociais 30, 79–97. https://doi.org/10.17666/308779-95/2015

Campbell, C. M., & Wiles, P. (1976). The study of law in society in Britain. Law & Society Review, 10(4), 547–578. https://doi.org/10.2307/3053297

Carey, G., & Crammond, B. (2015). Systems change for the social determinants of health. BMC Public Health, 15, 662. https://doi.org/10.1186/s12889-015-1979-8

Cash-Gibson, L., Rojas-Gualdrón, D. F., Pericàs, J. M., & Benach, J. (2018). Inequalities in global health inequalities research: A 50-year bibliometric analysis (1966-2015). PLOS ONE, 13(1), e0191901. https://doi.org/10.1371/journal.pone.0191901

Cesarini, D., Lindqvist, E., Östling, R., & Wallace, B. (2016). Wealth, health, and child development: Evidence from administrative data on Swedish lottery players. The Quarterly Journal of Economics, 131(2), 687-738. https://doi.org/10.1093/qje/qjw001

Chadwick, E. (1842). Report on the Sanitary Condition of the Labouring Population Og Great: Britain : Supplementary Report on the Results of Special Inquiry Into the Practice of Interment in Towns. H.M. Stationery Office.

Chang, W.-C., & Fraser, J. H. (2017). Cooperate! A paradigm shift for health equity. International Journal for Equity in Health, 16(1), 12. https://doi.org/10.1186/s12939-016-0508-4

Chubin, D. E. (1976). State of the Field The Conceptualization of Scientific Specialties. The Sociological Quarterly, 17(4), 448–476. https://doi.org/10.1111/j.1533-8525.1976.tb01715.x

Clark, B. R. (Ed.). (1987). The Academic Profession. National, Disciplinary & Institutional Settings. Berkeley: University of California Press.

Cole, S. (1994). Why sociology doesn't make progress like the natural sciences. Sociological Forum, 133–154.

Collyer, T. A. (2018). Three Metaphors to Aid Interdisciplinary Dialogue in Public Health. American Journal of Public Health, 108(11), 1483–1486. https://doi.org/10.2105/AJPH.2018.304681

Collyer, T.A., Smith, K.E. (2020). An atlas of health inequalities and health disparities research: "How is this all getting done in silos, and why?". Social Science & Medicine 264, 113330. https://doi.org/10.1016/j.socscimed.2020.113330

Craver, C. F. (2007). Explaining the brain: Mechanisms and the mosaic unity of neuroscience. Oxford University Press.

Creager, A. (2016). Paradigms and Exemplars Meet Biomedicine. In R. J. Richards & L. Daston (Eds.), Kuhn's 'Structure of Scientific Revolutions' at Fifty: Reflections on a Science Classic. Chicago, IL, United States: University of Chicago Press.

Crombie, I., Irvine, L., Elliott, L., & Hilary, W. (2004). Closing the Health Inequalities Gap: An International Perspective. Dundee: NHS Health Scotland and University of Dundee.

Dahlgren, G., & Whitehead, M. (1991). Policies and strategies to promote social equity in health. Background document to WHO-Strategy paper for Europe (No. 2007: 14). Institute for Futures Studies.

D'Angelo, C. A., Giuffrida, C., & Abramo, G. (2010). A heuristic approach to author name disambiguation in bibliometrics databases for large-scale research assessments. Journal of the American Society for Information Science and Technology, 62(2), 257–269. https://doi.org/10.1002/asi.21460

Daston, L., & Galison, P. (1992). The Image of Objectivity. Representations, (40), 81–128. https://doi.org/10.2307/2928741

De Battisti, F., & Salini, S. (2013). Robust analysis of bibliometric data. Statistical Methods & Applications, 22(2), 269–283. https://doi.org/10.1007/s10260-012-0217-0

De Lozano, S. R., & Cardenas, M. (2002). Some Learning Problems Concerning the Use of Symbolic Language in Physics. Science & Education, 11(6), 589–599. https://doi.org/10.1023/A:1019643420896

De Solla Price, D. J. (1963). Little science, big science (Vol. 5). Columbia University Press New York.

Dean, K., & Hunter, D. (1996). New directions for health: Towards a knowledge base for public health action. Social Science & Medicine, 42(5), 745–750. https://doi.org/10.1016/0277-9536(95)00394-0

Desrosières, A. (2007). Comparing the Incomparable : the Sociology of Statistics, in : T. Martin (ed) : Augustin Cournot : Modelling Economics, Edward Elgar, pp. 116-143.

Diez-Roux, A. V. (1998). Bringing context back into epidemiology: Variables and fallacies in multilevel analysis. American Journal of Public Health, 88(2), 216–222. https://doi.org/10.2105/ajph.88.2.216

Dixon-Woods, M., Yeung, K., & Bosk, C. L. (2011). Why is UK medicine no longer a self-regulating profession? The role of scandals involving "bad apple" doctors. Social Science & Medicine, 73(10), 1452–1459. http://doi.org/10.1016/j.socscimed.2011.08.031

Drees, L. A. (1982). "The Biglan Model: An Augmentation." Unpublished doctoral diss., The University of Nebraska.

du Bois-Reymond, E. (1882). The seven world problems'. Popular Science Monthly, 20, 433–447.

Duhem, P. M. M. (1954). The Aim and Structure of Physical Theory. Princeton: Princeton University Press.

Edwards, A. W. F. (1972). Likelihood. Cambridge University Press (Archive).

Elford, J., Whincup, P., & Shaper, A. G. (1991). Early Life Experience and Adult Cardiovascular Disease: Longitudinal and Case-Control Studies. International Journal of Epidemiology, 20(4), 833–844. https://doi.org/10.1093/ije/20.4.833

Elliott, E., Popay, J. and Williams, G. (2016). Knowledge of the everyday: confronting the causes of health inequalities. In K.E. Smith, C. Bambra and S.E. Hill (eds) Health Inequalities: Critical Perspectives. Oxford: Oxford University Press. http://doi.org/10.1093/acprof:oso/9780198703358.003.0016

Ellison, N., Heino, R., & Gibbs, J. (2006). Managing Impressions Online: Self-Presentation Processes in the Online Dating Environment. Journal of Computer-Mediated Communication, 11(2), 415–441. https://doi.org/10.1111/j.1083-6101.2006.00020.x

Ercan, I., Ocakoglu, G., Ozkaya, G., Sigirli, D., Cangür, S. (2013). An International Survey of Physicians' Knowledge of Biostatistics. Turkiye Klinikleri Journal of Medical Sciences 33, 401–409. https://doi.org/10.5336/medsci.2012-29673

Evans, C. (1995). Choosing people: Recruitment and selection as leverage on subjects and disciplines. Studies in Higher Education, 20(3), 253–265. https://doi.org/10.1080/03075079512331381535

Evetts, J. (2002). New directions in state and international professional occupations: Discretionary decision-making and acquired regulation. Work, Employment and Society, 16(2), 341–353.

Fam, Dena M., Tanzi Smith, and Dana J Cordell. (2017). "Being a transdisciplinary researcher: skills and dispositions fostering competence in transdisciplinary research and practice." In Transdisciplinary Research and Practice for Sustainability Outcomes, 122-136. Oxford: Routledge.

Fanelli, D., & Glänzel, W. (2013). Bibliometric Evidence for a Hierarchy of the Sciences. PLOS ONE, 8(6), e66938. https://doi.org/10.1371/journal.pone.0066938

Favero, M. D. (2006). Disciplinary variation in preparation for the academic dean role. Higher Education Research & Development, 25(3), 277–292. https://doi.org/10.1080/07294360600793069

Feller, I. (2006). Multiple actors, multiple settings, multiple criteria: Issues in assessing interdisciplinary research. Res Eval 15, 5–15. https://doi.org/10.3152/147154406781776020

Fiscella, K., Franks, P., Gold, M. R., & Clancy, C. M. (2000). Inequality in quality: addressing socioeconomic, racial, and ethnic disparities in health care. JAMA, 283(19), 2579-2584. https://doi.org/10.1001/jama.283.19.2579

Fontana, A., & Frey, J. H. (2000). The interview : From structured questions to negotiated text. In N. K. Denzin & Y. S. Lincoln (Eds.), Handbook of qualitative research (2nd ed, pp. 645–672). Thousand Oaks, Calif: Sage Publications.

Fourcade, M., Ollion, E., & Algan, Y. (2015). The Superiority of Economists. Journal of Economic Perspectives, 29(1), 89–114. https://doi.org/10.1257/jep.29.1.89

Fox Keller, E. (1986). Making gender visible in the pursuit of nature's secrets. In Feminist studies/Critical studies (pp. 67–77). Springer. https://doi.org/10.1007/978-1-349-18997-7_5

Frodeman, R. (2017). The Future of Interdisciplinarity. In The Oxford Handbook of Interdisciplinarity. http://doi.org/10.1093/oxfordhb/9780198733522.013.1

Funtowicz, S. O., & Ravetz, J. R. (1993). Science for the post-normal age. Futures, 25(7), 739–755. https://doi.org/10.1016/0016-3287(93)90022-L

Gagné, T., Lapalme, J., & McQueen, D. V. (2018). Multidisciplinarity in health promotion: A bibliometric analysis of current research. Health Promotion International, 33(4), 610–621. https://doi.org/10.1093/heapro/dax002

Gakidou, E. E., Murray, C. J. L., & Frenk, J. (2000). Defining and measuring health inequality: An approach based on the distribution of health expectancy. Bulletin of the World Health Organization, 78, 42–54.

Galea, S., & Link, B. G. (2013). Six Paths for the Future of Social Epidemiology. American Journal of Epidemiology, 178(6), 843–849. https://doi.org/10.1093/aje/kwt148

Galison, P. (1997). Image and Logic: A Material Culture of Microphysics. University of Chicago Press.

Galison, P. (1999). Trading Zone - Coordinating Action and Belief. In M. Biagioli (Ed.), The science studies reader. Psychology Press.

Galison, P. (2016). Practice all the way down. In Richards, R., Daston, L., (Eds.) Kuhn's' Structure of Scientific Revolutions' at Fifty: Reflections on a Science Classic. University of Chicago Press. Chicago. https://doi.10.7208/chicago/9780226317175.001.00001

Galton, S. F. (1875). English Men of Science: Their Nature and Nurture. Macmillan & Company.

Garthwaite, K., Smith, K.E., Bambra, C., Pearce, J. (2016). Desperately seeking reductions in health inequalities: Perspectives of UK researchers on past, present and future directions in health inequalities research. Sociol Health Illn 38, 459–478. https://doi.org/10.1111/1467-9566.12374

Gelman, A. (2016). The problems with p-values are not just with p-values. The American Statistician, 70(10), 1-2.

Gibbons, M. (1994). The new production of knowledge: The dynamics of science and research in contemporary societies. Sage. http://doi.org/10.4135/9781446221853

Giddens, A. (1976). Classical Social Theory and the Origins of Modern Sociology. American Journal of Sociology, 81(4), 703–729. https://doi.org/10.1086/226140

Gieryn, T. F. (1983). Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists. American Sociological Review, 48(6), 781–795. https://doi.org/10.2307/2095325

Gigerenzer, G. (2018). Statistical Rituals: The Replication Delusion and How We Got There. Advances in Methods and Practices in Psychological Science, 1(2), 198–218. https://doi.org/10.1177/2515245918771329

Gilbert, G. N., Gilbert, P. N., & Mulkay, M. (1984). Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse. Cambridge University Press.

Gmür, M. (2003). Co-citation analysis and the search for invisible colleges: A methodological evaluation. Scientometrics, 57(1), 27–57. https://doi.org/10.1023/A:1023619503005

Goffman, E. (1959). The presentation of self in everyday life. Harmondsworth, London.

Goldin, J., Zhu, W., Sayre, J.W. (1996). A review of the statistical analysis used in papers published in Clinical Radiology and British Journal of Radiology. Clinical Radiology 51, 47–50. https://doi.org/10.1016/S0009-9260(96)80219-4

Goldthorp, J. (2005). Progress in Sociology: The Case of Social Mobility Research. In S. Svallfors (Ed.), Analyzing Inequality: Life Chances and Social Mobility in Comparative Perspective (pp. 56–73). Stanford University Press.

Gorman, E. H., & Sandefur, R. L. (2011). "Golden Age," Quiescence, and Revival: How the Sociology of Professions Became the Study of Knowledge-Based Work. Work and Occupations, 38(3), 275–302. https://doi.org/10.1177/0730888411417565

Graham, H. (2009). Understanding health inequalities (Second edition.). Maidenhead: McGraw-Hill : Open University Press. https://doi.org/10.1332/030557309X445618

Granfield, R. (2007). The Meaning of Pro Bono: Institutional Variations in Professional Obligations among Lawyers. Law & Society Review, 41, 113. https://doi.org/10.1111/j.1540-5893.2007.00293.x

Greer, S. L. (2004). Territorial Politics and Health Policy: UK Health Policy in Comparative Perspective. Manchester University Press.

Grundmann, R. (2017). The Problem of Expertise in Knowledge Societies. Minerva, 55(1), 25–48. https://doi.org/10.1007/s11024-016-9308-7

Guendelman, S., & Abrams, B. (1995). Dietary intake among Mexican-American women: Generational differences and a comparison with white non-Hispanic women. American Journal of Public Health, 85(1), 20–25. https://doi.org/10.2105/AJPH.85.1.20

Gunderson, R. (2016). The sociology of technology before the turn to technology. Technology in Society, 47, 40–48. https://doi.org/10.1016/j.techsoc.2016.08.001

Hacking, I. (2012). Introduction. In The Structure of Scientific Revolutions: 50th Anniversary Edition. University of Chicago Press. https://doi.org/10.2307/23356448

Han, S.-K. (2003). Tribal regimes in academia: A comparative analysis of market structure across disciplines. Social Networks, 25(3), 251–280. https://doi.org/10.1016/S0378-8733(03)00011-X

Haraway, D. J. (1989). Primate visions: Gender, race, and nature in the world of modern science. Psychology Press.

Harper, S., Lynch, J., Meersman, S. C., Breen, N., Davis, W. W., & Reichman, M. E. (2008). An overview of methods for monitoring social disparities in cancer with an example using trends in lung cancer incidence by area-socioeconomic position and race-ethnicity, 1992–2004. American journal of epidemiology, 167(8), 889-899. https://doi.org/10.1093/aje/kwn016

Head, M. L., Holman, L., Lanfear, R., Kahn, A. T., & Jennions, M. D. (2015). The Extent and Consequences of P-Hacking in Science. PLOS Biology, 13(3), e1002106. https://doi.org/10.1371/journal.pbio.1002106

Hess, D. J. (1997). Science Studies: An Advanced Introduction. NYU Press.

Hessels, L. K., & van Lente, H. (2008). Re-thinking new knowledge production: A literature review and a research agenda. Research Policy, 37(4), 740–760. https://doi.org/10.1016/j.respol.2008.01.008

Hidalgo, B., & Goodman, M. (2012). Multivariate or Multivariable Regression? American Journal of Public Health, 103(1), 39–40. https://doi.org/10.2105/AJPH.2012.300897

Hoff, T., & McCaffrey, D. P. (1996). Adapting, Resisting, and Negotiating: How Physicians Cope with Organizational and Economic Change. Work and Occupations, 23(2), 165–189. https://doi.org/10.1177/0730888496023002003

Honjo, K. (2004). Social epidemiology: Definition, history, and research examples. Environmental Health and Preventive Medicine, 9(5), 193–199. https://doi.org/10.1007/BF02898100

House, J. S., & Williams, D. R. (1996). Psychosocial pathways linking SES and CVD. National Institutes of Health, National Heart, Lung, and Blood Institute, 119–124.

Huang, R. (2008). RQDA: R-based Qualitative Data Analysis. R package version 0.3.1. http://rqda.r-forge.r-project.org/

Hyland, K. (2004). Disciplinary Discourses, Michigan Classics Ed.: Social Interactions in Academic Writing. University of Michigan Press.

Illsley, R., & Le Grand, J. (1987). The Measurement of Inequality in Health. In A. Williams (Ed.), Health and Economics: Proceedings of Section F (Economics) of the British Association for the Advancement of Science, Bristol, 1986 (pp. 12–36). London: Palgrave Macmillan UK. https://doi.org/10.1007/978-1-349-18800-0_2

Jasanoff, S. (2003). Breaking the Waves in Science Studies: Comment on H.M. Collins and Robert Evans, 'The Third Wave of Science Studies'. Social Studies of Science, 33(3), 389–400. https://doi.org/10.1177/03063127030333004

Jasanoff, S. (2012). A field of its own: The emergence of science and technology studies. In R. Frodeman, J. T. Klein, & C. Mitcham (Eds.), The Oxford Handbook of Interdisciplinarity (Second Edition). Oxford, New York: Oxford University Press.

Jensenius, A. R. (2012, March 12). Disciplinarities: Intra, cross, multi, inter, trans. https://www.arj.no/2012/03/12/disciplinarities-2/

Jonnalagadda, S., & Topham, P. (2010). NEMO: Extraction and normalization of organization names from PubMed affiliation strings. Journal of Biomedical Discovery and Collaboration, 5, 50–75.

Jost, L. (2007). Partitioning Diversity into Independent Alpha and Beta Components. Ecology, 88(10), 2427–2439. https://doi.org/10.1890/06-1736.1

Jung, J., & Horta, H. (2013). Higher Education Research in Asia: a Publication and Co-Publication Analysis. Higher Education Quarterly, 67(4), 398-419. https://doi.org/10.1007/978-981-10-4989-7_20

Kaiser, M. I. (2019). Normativity in the Philosophy of Science. Metaphilosophy, 50(1-2), 36–62. https://doi.org/10.1111/meta.12348

Kakwani, N., Wagstaff, A., & van Doorslaer, E. (1997). Socioeconomic inequalities in health: Measurement, computation, and statistical inference. Journal of Econometrics, 77(1), 87–103. https://doi.org/10.1016/S0304-4076(96)01807-6

Kaplan, G. A., & Keil, J. E. (1993). Socioeconomic factors and cardiovascular disease: A review of the literature. Circulation, 88(4 Pt 1), 1973–1998. https://doi.org/10.1161/01.cir.88.4.1973

Kaplan, G. A., & Salonen, J. T. (1990). Socioeconomic conditions in childhood and ischaemic heart disease during middle age. BMJ, 301(6761), 1121–1123. https://doi.org/10.1136/bmj.301.6761.1121

Kawachi, I., & Kennedy, B. P. (1997). The relationship of income inequality to mortality: Does the choice of indicator matter? Social Science & Medicine, 45(7), 1121–1127. https://doi.org/10.1016/S0277-9536(97)00044-0

Kawachi, I., Subramanian, S. V., & Almeida-Filho, N. (2002). A glossary for health inequalities. Journal of Epidemiology & Community Health, 56(9), 647–652. https://doi.org/10.1136/jech.56.9.647

Kehm B.M., Teichler U. (2013) Organisational Strategy and the Profile of Cher Members. In: Kehm B.M., Musselin C. (eds) The Development of Higher Education Research in Europe. Higher Education Research in the 21st Century Series. SensePublishers, Rotterdam. https://doi.org/10.1007/978-94-6209-401-7_3

Kelly, M. P., & Charlton, B. G. (1992). Health promotion: Time for a new philosophy? The British Journal of General Practice, 42(359), 223–224.

Kerr, N. L. (1998). HARKing: Hypothesizing After the Results are Known. Personality and Social Psychology Review, 2(3), 196–217. https://doi.org/10.1207/s15327957pspr0203_4

Kindig, D. A. (2007). Understanding Population Health Terminology. The Milbank Quarterly, 85(1), 139–161. https://doi.org/10.1111/j.1468-0009.2007.00479.x

Klein, J. T. (1996). Crossing Boundaries: Knowledge, Disciplinarities, and Interdisciplinarities. University of Virginia Press.

Klein, J. T. (2012). A Taxonomy of Interdisciplinarity. In R. Frodeman & C. Mitcham (Eds.), The Oxford Handbook of Interdisciplinarity (Second Edition, p. Chaper 2). Oxford, New York: Oxford University Press.

Knorr Cetina, K., & Reichmann, W. (2015). Epistemic Cultures. In J. D. Wright (Ed.), International Encyclopedia of the Social & Behavioral Sciences (Second Edition) (pp. 873–880). Oxford: Elsevier. https://doi.org/10.1016/B978-0-08-097086-8.10454-4

Knorr-Cetina, K. (1999). Epistemic Cultures: How the Sciences Make Knowledge. Harvard University Press.

Kolb, D. A. (1981). Learning styles and disciplinary differences. The modern American college,1, 232-235.

Kosmützky, A., & Krücken, G. (2014). Growth or steady state? A bibliometric focus on international comparative higher education research. Higher Education, 67(4), 457–472. https://doi.org/10.1007/s10734-013-9694-9

Krieger, N. (1994). Epidemiology and the web of causation: Has anyone seen the spider? Social Science & Medicine (1982), 39(7), 887–903. https://doi.org/10.1016/0277-9536(94)90202-x

Krieger, N. (2000). Epidemiology and Social Sciences: Towards a Critical Reengagement in the 21st Century. Epidemiologic Reviews, 22(1), 155–163. https://doi.org/10.1093/oxfordjournals.epirev.a018014

Krieger, N., Chen, J. T., Waterman, P. D., Soobader, M. J., Subramanian, S. V., & Carson, R. (2002). Geocoding and monitoring of US socioeconomic inequalities in mortality and cancer incidence: does the choice of area-based measure and geographic level matter? the Public Health Disparities Geocoding Project. American journal of epidemiology, 156(5), 471-482.

Krishnan, A. (2009). What Are Academic Disciplines? Some Observations on the Disciplinarity vs. Interdisciplinarity Debate (Working Paper). National Centre for Research Methods.

Kuhn, T. S. (1962). The Structure of Scientific Revolutions. University of Chicago Press.

Kuhn, T.S. (1967). Paradigms and Theories in Scientific Research (Box 3, Folder 14). Massachusetts Institute of Technology, Institute Archides and Special Collections, Cambridge MA.

Kuhn, T. S. (1970). The Structure of Scientific Revolutions (2nd Ed.). University of Chicago Press.

Kuhn, T. S. (1991). The Trouble with the Historical Philosophy of Science. Presented at the Robert and Maurine Rothschild Distinguished Lecture.

Kuhn, T. S. (2000). Reflections on my Critics. In The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview (pp. 123–175). Chicago: University of Chicago Press.

Kuhn, T.S (2000a). Possible Worlds in the History of Science. In The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview (pp. 58–89). Chicago: University of Chicago Press.

Lakatos, I., & Musgrave, A. (Eds.). (1970). Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of

Science, London 1965. (pp. 59–90). Cambridge University Press.
https://doi.org/10.1017/CBO9781139171434.008

Lamont, M. (2009). How professors think: Inside the curious world of academic judgment. Cambridge, Mass: Harvard University Press.

Lane, N. (1999). Common sense, nonsense and statistics. Journal of the Royal Society of Medicine, 92(4), 202–205. https://doi.org/10.1177/014107689909200415

Last, J. M. (2001). A dictionary of epidemiology (Vol. 4).Oxford University Press.

Latour, B. (1986). Science in Action: How to Follow Scientists and Engineers Through Society. Harvard University Press.

Latour, B., & Woolgar, S. (1986). Laboratory Life: The Construction of Scientific Facts. Princeton University Press.

Leeper, M. A., Powell, E., & Werhane, P. H. (2010). Creating trading zones across continents and economies: 14 the female health company. Trading Zones and Interactional Expertise: Creating New Kinds of Collaboration, 281-288.
https://doi.org/10.7551/mitpress/9780262014724.003.0014

Le Grand, J. (1987). Inequalities in health: Some international comparisons. European Economic Review, 31(1), 182–191.
https://doi.org/10.1016/0014-2921(87)90030-4

Leicht, K. T., & Fennell, M. L. (2001). Professional Work: A Sociological Approach (1st Edition). Malden, Mass., USA: Wiley-Blackwell.

LERU [League of European Research Universities]. (2017). Position Paper: Interdisciplinarity and the 21st century research-intensive university. Leuven, Belgium

Leydesdorff, L. (2001). The Challenge of Scientometrics: The Development, Measurement, and Self-organization of Scientific Communications. Universal-Publishers.

Leydesdorff, L., & Milojević, S. (2012). Scientometrics. arXiv Preprint arXiv:1208.4566.

Leydesdorff, L., Carley, S., & Rafols, I. (2013). Global maps of science based on the new Web-of-Science categories. Scientometrics, 94(2), 589–593.
https://doi.org/10.1007/s11192-012-0784-8

Li, D., Okamoto, J., Liu, H., & Leischow, S. (2015). A bibliometric analysis on tobacco regulation investigators. BioData Mining, 8,(1).
https://doi.org/10.1186/s13040-015-0043-7

Lindvig, K. (2017). Creating Interdisciplinarity within Monodisciplinary Structures. PhD Thesis, University of Copenhagen.

Lundberg, O. (2020). Is lack of causal evidence linking socioeconomic position with health an 'inconvenient truth'? European Journal of Public Health, 30(4), 619–619. https://doi.org/10.1093/eurpub/ckaa004

Lyall, C. (2019). Being an Interdisciplinary Academic: How Institutions Shape University Careers. (C. Lyall, Ed.). Cham: Springer International Publishing. https://doi.org/10.1007/978-3-030-18659-3_3

Lyall, C., & Fletcher, I. (2013). Experiments in interdisciplinary capacity-building: The successes and challenges of large-scale interdisciplinary investments. Science and Public Policy, 40(1), 1–7. https://doi.org/10.1093/scipol/scs113

Lynch, J. (2017). Reframing inequality? The health inequalities turn as a dangerous frame shift. Journal of Public Health, 39(4), 653–660. https://doi.org/10.1093/pubmed/fdw140

Lynch, J. (2020). Regimes of Inequality: The Political Economy of Health and Wealth. Cambridge University Press.

Lynch, J. W., Kaplan, G. A., Pamuk, E. R., Cohen, R. D., Heck, K. E., Balfour, J. L., & Yen, I. H. (1998). Income inequality and mortality in metropolitan areas of the United States. American Journal of Public Health, 88(7), 1074–1080. https://doi.org/10.2105/AJPH.88.7.1074

Lynch, P., & Rivers, N. (2015). Thinking with Bruno Latour in Rhetoric and Composition. SIU Press.

Macintyre, S., Ellaway, A., & Cummins, S. (2002). Place effects on health: How can we conceptualise, operationalise and measure them? Social Science & Medicine, 55(1), 125–139. https://doi.org/10.1016/S0277-9536(01)00214-3

Mackenbach, J. P. (2020). Re-thinking health inequalities. European Journal of Public Health, 30(4), 615–615. https://doi.org/10.1093/eurpub/ckaa001

Mackenbach, J. P., van de Mheen, H., & Stronks, K. (1994). A prospective cohort study investigating the explanation of socio-economic inequalities in health in The Netherlands. Social Science & Medicine (1982), 38(2), 299–308.

Mackenbach, J. P. (2006). Health inequalities: Europe in profile. An independent expert report commissioned by the UK presidency of the EU. Rotterdam: Dept. of Public Health, Erasmus MC, 41.

Mackenbach, J., & De Jong, J. (2018). Health inequalities: An interdisciplinary exploration of socioeconomic position, health and causality. Amsterdam: FEAM/ALLEA Committee on Health Inequalities.

MacKenzie, D. A. (1981). Statistics in Britain, 1865-1930: The Social Construction of Scientific Knowledge. Edinburgh University Press.

MacKenzie, D. (1981a). Sociobiologies in Competition: The Biometrician–Mendelian Debate. In C. Webster (Ed.), Biology, Medicine and Society 1840–1940 (pp. 243–288). Cambridge University Press. https://doi.org/10.1017/CBO9780511562822.009

MacKenzie, D., & Wajcman, J. (1999). The social shaping of technology. Open university press.

Mäki, U. (2001). The Economic World View: Studies in the Ontology of Economics. Cambridge University Press.

Mansilla, V.B., Feller, I., Gardner, H. (2006). Quality assessment in interdisciplinary research and education. Res Eval 15, 69–74. https://doi.org/10.3152/147154406781776057

Manor, O., Matthews, S., & Power, C. (1997). Comparing measures of health inequality. Social Science & Medicine, 45(5), 761–771. https://doi.org/10.1016/S0277-9536(96)00412-1

Marmot, M. (2001). From Black to Acheson: Two decades of concern with inequalities in health. A celebration of the 90th birthday of Professor Jerry Morris. International Journal of Epidemiology, 30(5), 1165–1171. https://doi.org/10.1093/ije/30.5.1165

Marmot, M., Allen, J., Bell, R., Bloomer, E., & Goldblatt, P. (2012). WHO European review of social determinants of health and the health divide. The lancet, 380(9846), 1011-1029.

Marmot, M. (2017). Social justice, epidemiology and health inequalities. European Journal of Epidemiology, 32(7), 537–546. https://doi.org/10.1007/s10654-017-0286-3

Marmot, M. G., Shipley, M. J., & Rose, G. (1984). Inequalities in death – Specific explanations of a general pattern? The Lancet, 323(8384), 1003–1006. https://doi.org/10.1016/S0140-6736(84)92337-7

Masterman, M. (1970). The Nature of a Paradigm. In I. Lakatos (Ed.), Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London. (pp. 59–90). Cambridge University Press. https://doi.org/10.1017/CBO9781139171434.008

Maton, K., & Muller, J. (2007). A sociology for the transmission of knowledges. Language, Knowledge and Pedagogy: Functional Linguistic and Sociological Perspectives, 14–33.

Matthews, R., Wasserstein, R., & Spiegelhalter, D. (2017). The ASA's p-value statement, one year on. Significance, 14(2), 38–41. https://doi.org/10.1111/j.1740-9713.2017.01021.x

McCulloch, A. (2001). Ward-Level Deprivation and Individual Social and Economic Outcomes in the British Household Panel Study. Environment and Planning A: Economy and Space, 33(4), 667–684. https://doi.org/10.1068/a33205

McCulloch, A. (2001a). Reply: Ward-level deprivation and individual social and economic outcomes in the British Household Panel Study. Environment and Planning A, 33(4), 1365–1369.

McPherson, K., Wennberg, J. E., Hovind, O. B., & Clifford, P. (1982). Small-area variations in the use of common surgical procedures: an international comparison of New England, England, and Norway. New England journal of medicine, 307(21), 1310-1314. https://doi.org/10.1056/nejm198211183072104

Mechanic, D. (1993). Social research in health and the American sociopolitical context: The changing fortunes of medical sociology. Social Science & Medicine, 36(2), 95–102. https://doi.org/10.1016/0277-9536(93)90200-N

Merigó, J. M., & Núñez, A. (2016). Influential journals in health research: A bibliometric study. Globalization and Health, 12(1), 46. https://doi.org/10.1186/s12992-016-0186-4

Merton, R. K. (1958). The functions of the professional association. The American Journal of Nursing, 58(1)50–54.

Mongeon, P., & Paul-Hus, A. (2016). The journal coverage of Web of Science and Scopus: A comparative analysis. Scientometrics, 106(1), 213–228. https://doi.org/10.1007/s11192-015-1765-5

Murray, C. J. L., Gakidou, E. E., & Frenk, J. (2000). Response to P. Braveman et al. Bulletin of the World Health Organization, 78, 236–237. https://doi.org/10.1590/S0042-96862000000200012

National Cancer Institute. SEER registries—about SEER. https://seer.cancer.gov/registries/index.html (2019), Accessed 27th Jul 2019

Nelson, A. (2002). Unequal treatment: confronting racial and ethnic disparities in health care. Journal of the National Medical Association, 94(8), 666.

Nerur, S. P., Rasheed, A. A., & Natarajan, V. (2008). The intellectual structure of the strategic management field: An author co-citation analysis. Strategic Management Journal, 29(3), 319–336. https://doi.org/10.1002/smj.659

Neumann, R., & Becher, T. (2002). Teaching and Learning in Their Disciplinary Contexts: A Conceptual Analysis. Studies in Higher Education - STUD HIGH EDUC, 27, 405–417. https://doi.org/10.1080/0307507022000011525

Norris, M., & Oppenheim, C. (2007). Comparing alternatives to the Web of Science for coverage of the social sciences' literature. Journal of Informetrics, 1(2), 161–169. https://doi.org/10.1016/j.joi.2006.12.001

Nowak, A. J. (2011). Paradigm shift: Infant oral health care – Primary prevention. Journal of Dentistry, 39, S49–S55. https://doi.org/10.1016/j.jdent.2011.11.005

Nowotny, H., Scott, P., & Gibbons, M. (2003). Introduction: "Mode 2" Revisited: The New Production of Knowledge. Minerva, 41(3), 179–194. https://doi.org/10.1023/A:1025505528250

Otero, H. (2018). Socio-history of statistics on Latin America: A review. Histoire et Mesure 33, 13–32. https://doi.org/10.4000/histoiremesure.7934

Pantazis, C., Gordon, D., & Levitas, R. (2006). Poverty and social exclusion in Britain: The millennium survey. Policy Press.

Parsons, T. (1951). Illness and the role of the physician: A sociological perspective. American Journal of Orthopsychiatry, 21(3), 452. https://doi.org/10.1111/j.1939-0025.1951.tb00003.x

Paulsen, M. B., & Wells, C. T. (1998). Domain differences in the epistemological beliefs of college students. Research in Higher Education, 39(4), 365–384. https://doi.org/10.1023/a:1018785219220

Pearce, N. (1996). Traditional epidemiology, modern epidemiology, and public health. American Journal of Public Health, 86(5), 678–683. https://doi.org/10.2105/ajph.86.5.678

Pearce, J. (2013). An environmental justice framework for understanding neighbourhood inequalities in health and well-being. In Van Ham, M., Manley, D., Maclennan, D., Bailey N, Simpson L (eds). Neighbourhood effects or neighbourhood based problems? A policy context. Springer Press, pp 89-111.

Pearce, J., Mitchell, R., & Shortt, N. (2016). Place, Space and Health Inequalities. In: Smith, K. E., Bambra, C. and Hill, S. E. (eds.) Health Inequalities: Critical Perspectives. Oxford University Press: Oxford, pp. 192-205.

Pernet, C. (2016). Null hypothesis significance testing: A short tutorial. F1000Research, 4. https://doi.org/10.12688/f1000research.6963.3

Petticrew, M., & Roberts, H. (2003). Evidence, hierarchies, and typologies: Horses for courses. Journal of Epidemiology & Community Health, 57(7), 527–529. https://doi.org/10.1136/jech.57.7.527

Piaget, J. (1967). Biologie et connaissance. Paris: Gallimard.

Pickersgill, M., & Jasanoff, S. (2018). ST(&)S: Martyn Pickersgill Talks with Sheila Jasanoff. Engaging Science, Technology, and Society, 4(0, 0), 320–334. https://doi.org/10.17351/ests2018.226

Pilkington, P., Powell, J., & Davis, A. (2016). Evidence-Based Decision Making When Designing Environments for Physical Activity: The Role of Public Health. Sports Medicine, 46(7), 997–1002. https://doi.org/10.1007/s40279-015-0469-6

Pinch, T. J., & Bijker, W. E. (2016). The Social Construction of Facts and Artefacts: Or How the Sociology of Science and the Sociology of Technology might Benefit Each Other: Social Studies of Science. https://doi.org/10.1177/030631284014003004

Popay, J., Williams, G., Thomas, C., & Gatrell, T. (1998). Theorising Inequalities in Health: The Place of Lay Knowledge. Sociology of Health & Illness, 20(5), 619–644. https://doi.org/10.1111/1467-9566.00122

Popper, K. (1970). Normal Science and its Dangers. In I. Lakatos & A. Musgrave (Eds.), Criticism and the Growth of Knowledge (pp. 51–58). Cambridge University Press. https://doi.org/10.1017/cbo9781139171434.007

Porter, A.L., Chubin, D.E. (1985). An indicator of cross-disciplinary research. Scientometrics 8, 161–176. https://doi.org/10.1007/BF02016934

Porter, A. L., & Rafols, I. (2009). Is science becoming more interdisciplinary? Measuring and mapping six research fields over time. Scientometrics, 81(3), 719. https://doi.org/10.1007/s11192-008-2197-2

Prévost, J.G. (2009). A total science: Statistics in liberal and Fascist Italy (pp. 1–335). McGill-Queen's University Press, Montreal.

Rafols, I., & Meyer, M. (2007). How cross-disciplinary is bionanotechnology? Explorations in the specialty of molecular motors. Scientometrics, 70(3), 633–650. https://doi.org/10.1007/s11192-007-0305-3

Ram, A. (1991). A Theory of Questions and Question Asking. Journal of the Learning Sciences 1, 273–318. https://doi.org/10.1080/10508406.1991.9671973

Relton, C. L., & Smith, G. D. (2010). Epigenetic Epidemiology of Common Complex Disease: Prospects for Prediction, Prevention, and Treatment. PLOS Medicine, 7(10), e1000356. https://doi.org/10.1371/journal.pmed.1000356

Reubi, D. (2017). A genealogy of epidemiological reason: Saving lives, social surveys and global population. BioSocieties, 1–22. https://doi.org/10.1057/s41292-017-0055-2

Richards, R. J., & Daston, L. (Eds.). (2016). Kuhn's 'Structure of Scientific Revolutions' at Fifty: Reflections on a Science Classic. Chicago, IL, UNITED STATES: University of Chicago Press.

Richter, M. (2010). It does take two to tango! On the need for theory in research on the social determinants of health. International Journal of Public Health, 55(5), 457–458. https://doi.org/10.1007/s00038-010-0148-1

Rip, A. (1988). Chapter 9 - Mapping of Science: Possibilities and Limitations. In A. F. J. Van raan (Ed.), Handbook of Quantitative Studies of Science and Technology (pp. 253–273). Amsterdam: Elsevier. https://doi.org/10.1016/B978-0-444-70537-2.50014-3

Robertson, I. (2003). An application of Basil Bernstein to vocational education and training policy in Australia. Educational Research, Risks and Dilemmas, Auckland.

Rotta, R., & Noack, A. (2011). Multilevel Local Search Algorithms for Modularity Clustering. J. Exp. Algorithmics, 16, 2.3:2.1–2.3:2.27. https://doi.org/10.1145/1963190.1970376

Rubin, M. (2017). When Does HARKing Hurt? Identifying When Different Types of Undisclosed Post Hoc Hypothesizing Harm Scientific Progress. https://doi.org/10.31234/osf.io/un372

Rudwick, J. (1985). The Great Devonian Controversy. University of Chicago Press.

Ruscio, K. (1987). Many Sectors, Many Professions. In B. R. Clark (Ed.), The Academic Profession. National, Disciplinary & Institutional Settings (pp. 331–368). Berkeley: University of California Press.

Sá, C. M. (2008). 'Interdisciplinary strategies' in U.S. research universities. Higher Education, 55(5), 537–552. https://doi.org/10.1007/s10734-007-9073-5

Sabri, D. (2013). Student evaluations of teaching as 'fact-totems': The case of the UK national student survey. Sociological Research Online 18. https://doi.org/10.5153/sro.3136

Schleef, D.J. (2005). Managing Elites: Socializaton in Law and Business Schools. Lanham: Rowman & Littlefield Publishers.

Sciulli, D. (2009). Professions in civil society and the state: Invariant foundations and consequences. Brill.

Schyfter, P., & MacKenzie, D. (2018). STS and its Challenging Obligations: Pablo Schyfter Talks with Donald MacKenzie. Engaging Science, Technology, and Society, 4(0), 335–351. https://doi.org/10.17351/ests2018.206

Senra, N., 2008. Historical study of statistics: Topics and sources [Pesquisa histórica das estatísticas: Temas e fontes]. Historia, Ciencias, Saude - Manguinhos 15, 411–425. https://doi.org/10.1590/S0104-59702008000200011

Shapin, S. (1995). Here and Everywhere: Sociology of Scientific Knowledge. Annual Review of Sociology, 21(1), 289–321. https://doi.org/10.1146/annurev.so.21.080195.001445

Shapin, S., & Schaffer, S. (1985). Leviathan and the Air-Pump. Princeton, New Jersey: Princeton University Press.

Shim, J. K. (2002). Understanding the routinised inclusion of race, socioeconomic status and sex in epidemiology: The utility of concepts from technoscience studies. Sociology of Health & Illness, 24(2), 129–150. https://doi.org/10.1111/1467-9566.00288

Shin, J., Lee, S., & Yoon, J. (2018). Bio Art as a Trading Zone: A Creolized Art Form of Biology and Art. In. Presented at the ISEA 2018, Durban: ISEA Symposium Archives.

Simpson, A. (2017). The surprising persistence of Biglan's classification scheme. Studies in Higher Education, 42(8), 1520–1531. https://doi.org/10.1080/03075079.2015.1111323

Singh, G. K., Miller, B. A., Hankey, B. F., & Edwards, B. K. (2004). Persistent area socioeconomic disparities in US incidence of cervical cancer, mortality, stage, and survival, 1975–2000. Cancer, 101(5), 1051-1057.

Skole, D.L. (2004). Geography as a Great Intellectual Melting Pot and the Preeminent Interdisciplinary Environmental Discipline. Annals of the Association of American Geographers 94, 739–743. https://doi.org/10.1111/j.1467-8306.2004.00429.x

Slattery, M. L., Lundgreen, A., Torres-Mejia, G., Wolff, R. K., Hines, L., Baumgartner, K., & John, E. M. (2014). Diet and lifestyle factors modify immune/inflammation response genes to alter breast cancer risk and prognosis: The Breast Cancer Health Disparities Study. Mutation Research, 770, 19–28. https://doi.org/10.1016/j.mrfmmm.2014.08.009

Smart, J. C., & Elton, C. F. (1982). Validation of the Biglan model. Research in Higher Education, 17(3), 213–229. https://doi.org/10.1007/BF00976699

Smith, K. (2010). Research, policy and funding – academic treadmills and the squeeze on intellectual spaces1. The British Journal of Sociology, 61(1), 176–195. https://doi.org/10.1111/j.1468-4446.2009.01307.x

Smith, K. E. (2007). Health inequalities in Scotland and England: The contrasting journeys of ideas from research into policy. Social Science & Medicine, 64(7), 1438–1449. https://doi.org/10.1016/j.socscimed.2006.11.008

Smith, K. E. (2008). Health inequalities in Scotland and England: The translation of ideas between research and policy. PhD Thesis. University of Edinburgh School of Social and Political Science. https://www.era.lib.ed.ac.uk/handle/1842/7203

Smith, K. E. (2015). Understanding responses to the political context of health inequalities in research and policy: Can post-structural theories of power help? Social Theory & Health, 13(3-4), 355–376. https://doi.org/10.1057/sth.2015.24

Smith, K. E., & Eltanani, M. K. (2014). What kinds of policies to reduce health inequalities in the UK do researchers support? Journal of Public Health, 37(1) 6-17. https://doi.org/10.1093/pubmed/fdu057

Smith, K. E., & Joyce, K. E. (2012). Capturing complex realities: Understanding efforts to achieve evidence-based policy and practice in public health. Evidence & Policy: A Journal of Research, Debate and Practice, 8(1), 57–78. https://doi.org/10.1332/174426412X6201371

Soler, L., Zwart, S., Lynch, M., & Israel-Jost, V. (2014). Science after the practice turn in the philosophy, history, and social studies of science. Routledge.

Somers, M.R. (1994) The narrative constitution of identity: a relational and network approach, Theory and Society, 23, 605–49. https://doi.org/10.1007/bf00992905

Soteriades, E.S., & Falagas, M. E. (2006). A bibliometric analysis in the fields of preventive medicine, occupational and environmental medicine, epidemiology, and public health. BMC Public Health, 6, 301. https://doi.org/10.1186/1471-2458-6-301

Spellerberg, I. F., & Fedor, P. J. (2003). A tribute to Claude Shannon (1916–2001) and a plea for more rigorous use of species richness, species diversity and the 'Shannon–Wiener'Index. Global Ecology and Biogeography, 12(3), 177–179. https://doi.org/10.1046/j.1466-822x.2003.00015.x

Spurling, N. (2012). Research Strategies, 'Capital' and the goods of social science research. In P. Trowler, M. Saunders, & V. Bamber (Eds.), Tribes and territories in the 21st century: Rethinking the significance of disciplines in higher education. Routledge.

Steckler, A., & McLeroy, K. R. (2008). The Importance of External Validity. American Journal of Public Health, 98(1), 9–10. https://doi.org/10.2105/AJPH.2007.126847

Stember, M. (1991). Advancing the social sciences through the interdisciplinary enterprise. The Social Science Journal, 28(1), 1–14. https://doi.org/10.1016/0362-3319(91)90040-B

Stichweh, R. (2001). History of scientific disciplines. International Encyclopedia of the Social and Behavioral Sciences, 13727–13731.

Stirling, A. (2007). A general framework for analysing diversity in science, technology and society. Journal of The Royal Society Interface, 4(15), 707–719. https://doi.org/10.1098/rsif.2007.0213

Stoecker, J. L. (1993). The biglan classification revisited. Research in Higher Education, 34(4), 451–464. https://doi.org/10.1007/bf00991854

Stott, N. C., Kinnersley, P., & Rollnick, S. (1994). The limits to health promotion. BMJ : British Medical Journal, 309(6960), 971–972. https://doi.org/10.1136/bmj.309.6960.971

Stuckler, D., Reeves, A., Karanikolos, M., & McKee, M. (2015). The Health Effects of the Global Financial Crisis: Can We Reconcile the Differing Views: A Network Analysis of Literature across Disciplines. Health Economics Policy and Law, 10, 83-99. https://doi.org/10.1017/s1744133114000255

Sugimoto, C. R., & Larivière, V. (2018). Measuring Research: What Everyone Needs to Know. Oxford University Press.

Sullivan, J. R. (2012). Skype: an appropriate method of data collection for qualitative interviews?. The Hilltop Review, 6(1), 10.

Susser, M. (1985). Epidemiology in the United States after World War II: The evolution of technique. Epidemiologic Reviews, 7(1), 147–177. https://doi.org/10.1093/oxfordjournals.epirev.a036280

Swales, J. M. (2001). EAP-related linguistic research: An intellectual history. Research Perspectives on English for Academic Purposes, 42–54. https://doi.org/10.1017/cbo9781139524766.006

Syme, L. (1996). To prevent disease: The need for a new approach. In D. Blane, E. Brunner, & R. Wilkinson (Eds.), Health and Social Organisation. London: Routledge.

Syme, S. L. (2007). The prevention of disease and promotion of health: The need for a new approach. European Journal of Public Health, 17(4), 329–330. https://doi.org/10.1093/eurpub/ckm081

Taylor, P. J. (1976). An Interpretation of the Quantification Debate in British Geography. Transactions of the Institute of British Geographers, 1(2), 129–142. https://doi.org/10.2307/621979

Teichler, U. (2015). Higher Education Research in Europe. In A. Curaj, L. Matei, R. Pricopie, J. Salmi, & P. Scott (Eds.), The European Higher Education Area: Between Critical Reflections and Future Policies (pp. 815–847). Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-20877-0_50

Thagard, P. (2018). How Scientists Explain Disease. Princeton University Press.

Thévenot, L. (2011). Conventions for measuring and questioning policies. The case of 50 years of policy evaluations through a statistical survey. Historical Social Research, 36(4), 192–217.

Tight, M. (2012). Researching higher education (2nd ed.). Maidenhead, Berkshire, England ; New York: Society for Research into Higher Education & Open University Press.

Tight, M. (2020). Higher education: Discipline or field of study? Tertiary Education and Management. https://doi.org/10.1007/s11233-020-09060-2

Torre Medina-Mora, P., & Salas Valenzuela, M. (2013). [A bibliometric review of infant feeding in Mexico]. Gaceta Medica De Mexico, 149(1), 53–60.

Trahair, E. (2014). Editorial. Planet: The Welsh Internationalist. 213(Spring):6-9

Trowler, P., Saunders, M., & Bamber, V. (Eds.). (2012). Tribes and Territories in the 21st Century: Rethinking the Significance of Disciplines in Higher Education. Taylor & Francis Group.

Trowler, P. (2014). Academic tribes and territories: The theoretical trajectory. Osterreichische Zeitschrift Fur Geschichtswissenschaften [Austrian Journal of History], 25, 17–26.

Truffer, B. (2008). Society, Technology, and Region: Contributions from the Social Study of Technology to Economic Geography. Environment and Planning A: Economy and Space, 40(4), 966–985. https://doi.org/10.1068/a39170

Van Eck, N. J., & Waltman, L. (2014). Visualizing Bibliometric Networks. In Measuring Scholarly Impact (pp. 285–320). Springer, Cham. https://doi.org/10.1007/978-3-319-10377-8_13

Wagstaff, A., & Culyer, A. J. (2012). Four decades of health economics through a bibliometric lens. Journal of Health Economics, 31(2), 406–439. https://doi.org/10.1016/j.jhealeco.2012.03.002

Walsh, D., McCartney, G., Minton, J., Parkinson, J., Shipton, D., & Whyte, B. (2020). Changing mortality trends in countries and cities of the UK: A population-based trend analysis. BMJ Open, 10(11), e038135. https://doi.org/10.1136/bmjopen-2020-038135

Walsham, G. (1995). The Emergence of Interpretivism in IS Research. Information Systems Research 6, 376–394.

Waltman, L., & Van Eck, N. J. (2013). A smart local moving algorithm for large-scale modularity-based community detection. The European Physical Journal B, 86(11), 471. https://doi.org/10.1140/epjb/e2013-40829-0

Wang, Q., & Waltman, L. (2016). Large-scale analysis of the accuracy of the journal classification systems of Web of Science and Scopus. Journal of Informetrics, 10(2), 347–364. https://doi.org/10.1016/j.joi.2016.02.003

Wasserstein, R. L., & Lazar, N. A. (2016). The ASA's Statement on p-Values: Context, Process, and Purpose. The American Statistician, 70(2), 129–133. https://doi.org/10.1080/00031305.2016.1154108

Watkins, J. (1970). Against "normal science". In Criticism and the growth of knowledge: Volume 4: Proceedings of the international colloquium in the philosophy of science, london (p. 25). Cambridge University Press.

Weingart, P. (2012). A short History of Knowledge Formations. In R. Frodeman, J. T. Klein, & C. Mitcham (Eds.), The Oxford Handbook of Interdisciplinarity (Second Edition). Oxford, New York: Oxford University Press.

Weingart, P. (2014). Interdisciplinarity and the new governance of universities. University Experiments in Interdisciplinarity: Obstacles and Opportunities, 151–174. doi:10.14361/transcript.9783839426166.151

Whitehead, M. (1987). The Health Divide Inequalities in Health in the 1980's, a Review Commissioned by the Health Education Council, London. London Health Education Authority.

Williams, D. R., & Mohammed, S. A. (2008). Discrimination and racial disparities in health: Evidence and needed research. Journal of Behavioral Medicine, 32(1), 20–47. https://doi.org/10.1007/s10865-008-9185-0

Williams, G. H. (2003). The determinants of health: Structure, context and agency. Sociology of Health & Illness, 25(3), 131–154. https://doi.org/10.1111/1467-9566.00344

Woodward, J. (2005). Making Things Happen: A Theory of Causal Explanation. Oxford University Press, USA.

Woolgar, S. (1991). The Turn to Technology in Social Studies of Science: Science, Technology, & Human Values. https://doi.org/10.1177/016224399101600102

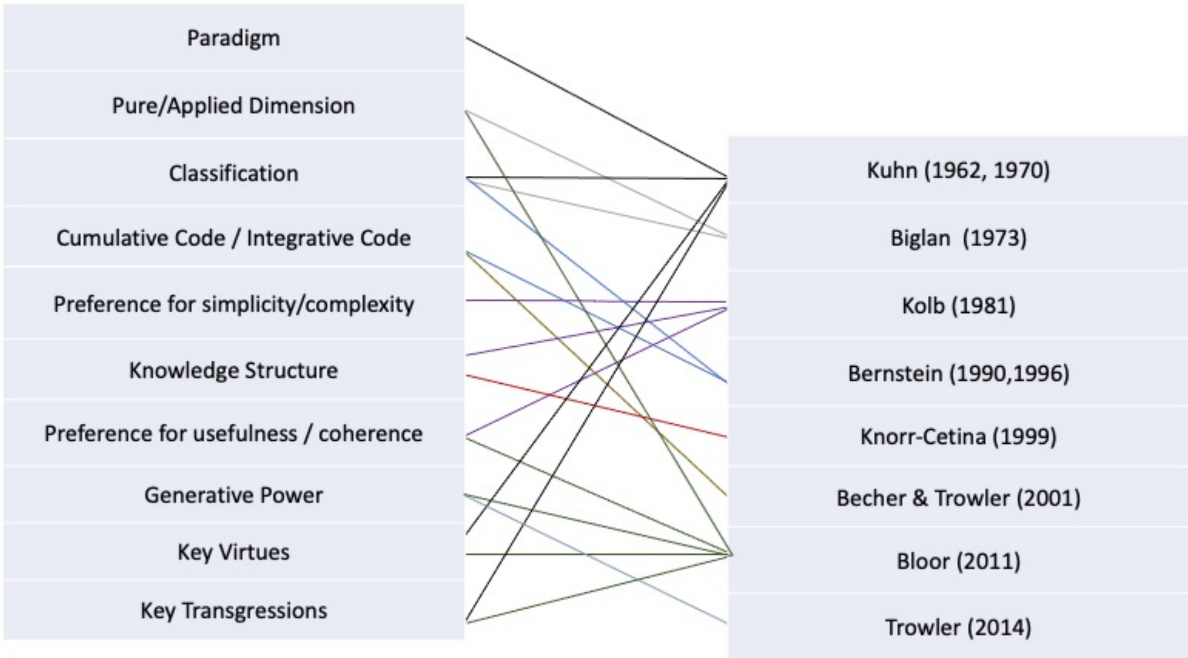
World Health Organization. (2000). The World Health Report 2000: Health Systems : Improving Performance. World Health Organization.

Zeigler, E.F. (1990). Don’t forget the profession when choosing a name. The Evolving Undergraduate Major, 67–77.

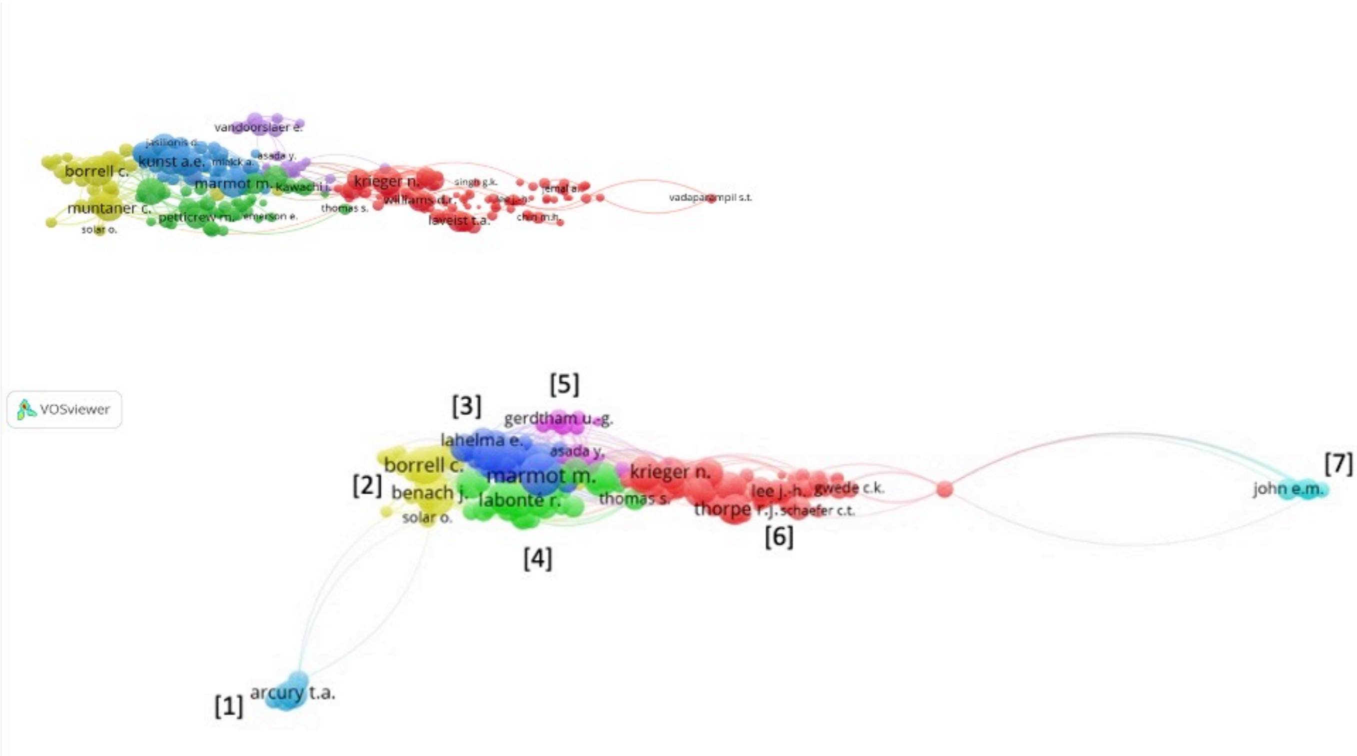
APPENDIX A - INTERVIEW SCHEDULE

Interview Schedule		
0	INTRODUCE PROJECT	CONSENT / WITHDRAWAL / ANONYMISATION
	BACKGROUND	
1A	Overview of Academic Training	**How do you describe yourself in disciplinary terms?**
		Multiple identities- how do they manage this?
		No disciplinary ID - How has training impacted their work?
1B	Own Discipline	**Any concept from wish others understood better?**
	CAUSATION	
Spiel		I'd like to … **your own personal mental-model of health and illness**… According to your own outlook, **health and disease are distributed.**
2A	Mental Model	Generally, **what do you feel is the major process... (or set) determining whether a person becomes sick or is will?**
2B	Tension between levels	
		How do you view the **interaction of individual and social causes of illness**?
		How should these different causes be **integrated in research**?
	ENGAGEMENT	
3A		Do you regularly **engage w researchers** from other disc?
3B		What have attempts to collaborate been like?
3C		Are there any **kinds of research which frustrate you**?
	METHODOLOGY	
4A	Funding Activity	*Pick exposure & Rank study designs*
4B	Sense of Good Research	**What … hallmarks of good empirical research?**
(4C)	Sense of bad research	**Red flags**… suggests **lacking or inferior**?
	REGRESSION	Not a test or attempt to gauge proficiency, **interested in your own understanding**
XA		If a colleague asked, **what do regression models do?**
XB		Do these models **represent reality?** (If not, what?)
XC		**What are the coefficients? Discovery? Construction?**
XD		P values & NHST if time
	CLOSING OUT	
5A	Additional comments	Anything important we've not touched on?
5B		Anything you'd like to add to what you've said already?
5C	Giving Feedback	Would you like to make any comments on how this interview has been conducted?
5E	Getting Feedback	Would you like to receive feedback about the outcome of the research? **CHECK HOW**

Appendix B: Literature underpinning the disciplinary matrix



APPENDIX C: PREVIOUS VERSIONS OF THE BIBLIOMETRIC NETWORK



APPENDIX D: AUTHORS INCLUDED IN THE BIBLIOMETRIC NETWORK

Author	Cluster	Total link strength^	Documents*	Citations^
acevedo-garcia d.	1	151	17	608
adler n.	1	258	44	3724
arber s.	2	182	14	812
arcury t.a.	5	303	52	1195
artazcoz l.	5	212	21	463
asada y.	4	187	19	268
bambra c.	3	1101	57	2028
barros a.j.d.	4	68	22	582
barros f.c.	4	108	11	1004
bartley m.	2	440	27	1066
bauld l.	3	121	11	745
baum f.	3	376	63	1027
baumgartner k.b.	6	362	22	261
beckfield j.	4	204	10	239
bell c.n.	1	189	11	159
bell r.	4	99	20	1701
benach j.	5	854	53	1376
blackman t.	3	111	10	194
blakely t.	7	262	38	1295
blane d.	2	127	16	567
bleich s.n.	1	131	7	260
borrell c.	3	839	72	1438
borrell l.n.	1	98	17	487
bowie j.v.	1	250	17	141
boyce w.t.	2	255	13	2441
brandt h.m.	6	43	15	269
braveman p.	1	566	31	3379
breen n.	4	170	11	1157
browne a.j.	1	48	19	343
bruce m.a.	1	127	11	243
brunner e.j.	2	405	19	2955
burström b.	3	162	21	330
chandola t.	2	209	18	633
chen e.	1	99	16	707
chen h.	5	158	18	320
chen j.	1	583	27	2720
chin m.h.	1	229	37	1229
chung h.	5	462	27	353
clarke p.m.	4	252	17	719
coburn d.	3	165	7	563
collins c.	1	134	12	709

cooper h.	1	85	18	575
cooper l.a.	1	85	26	1122
croft j.b.	1	59	9	834
cummins s.	2	101	13	907
currie c.	2	249	14	1267
curtis s.	2	148	17	1139
dahl e.	2	420	21	665
dean h.d.	1	75	16	298
diderichsen f.	2	181	24	647
díez e.	3	160	9	85
dorling d.	7	243	20	526
dovidio j.f.	1	68	11	700
dunn j.r.	2	157	24	903
duran b.	1	59	8	1123
edwards b.k.	8	103	5	1031
egan m.	3	207	8	422
egerter s.	1	156	7	1012
eikemo t.a.	2	416	18	617
elgar f.j.	2	143	14	206
elstad j.i.	2	257	17	536
emerson e.	3	163	29	814
emmons k.m.	1	115	17	452
erreygers g.	4	134	6	322
espelt a.	5	256	10	253
espey d.k.	8	116	9	634
ezzati m.	4	108	12	1220
firth d.	2	141	6	296
fiscella k.	1	100	22	878
fisher m.	3	119	16	175
fitzpatrick r.	2	153	7	315
frenk j.	4	98	10	784
friel s.	3	173	42	791
fritzell j.	2	272	14	371
frohlich k.l.	2	164	17	1042
fukuda y.	2	142	14	269
gary-webb t.l.	1	131	9	1265
gaskin d.j.	1	336	16	560
gee g.c.	1	144	14	998
gerdtham u.-g.	4	238	22	532
geronimus a.t.	1	107	17	764
gibson m.	3	115	5	304
giuliano a.r.	6	195	17	197

graham h.	3	361	19	1266
griffith d.m.	1	264	26	374
grundy e.	2	126	12	610
gruskin s.	1	129	7	732
grzywacz j.g.	5	235	27	568
gwede c.k.	6	72	18	185
halfon n.	1	38	15	998
harper s.	4	481	40	1304
harris r.	8	88	11	380
hatton c.	3	117	15	458
hatzenbuehler m.l.	1	89	22	1581
hébert j.r.	6	65	12	61
hicken m.t.	1	170	10	185
hines l.	6	426	25	311
hiscock r.	7	155	9	732
hoffmann r.	3	193	13	114
hosseinpoor a.r.	4	80	16	330
house j.s.	1	221	9	1162
houweling t.a.j.	4	119	11	462
huang e.s.	1	175	17	894
huisman m.	4	166	5	291
hunter d.j.	3	175	19	362
hurrelmann k.	2	252	13	86
islam n.	1	119	20	247
israel b.a.	1	128	18	992
jackson j.s.	1	343	23	955
james s.a.	1	92	10	416
jemal a.	8	138	24	22863
jim m.a.	8	123	9	662
john e.m.	6	433	27	287
joyce k.e.	3	181	8	394
judge k.	3	202	13	612
kaplan g.a.	2	344	11	2516
kaufman j.s.	4	138	30	590
kawachi i.	1	457	74	2536
kershaw k.n.	1	151	13	319
khang y.-h.	2	155	14	450
king n.	4	175	12	220
krieger n.	1	1166	60	5483
kristjansson e.	3	140	11	272
kumanyika s.k.	1	82	26	847

kunst a.e.	4	826	54	1979
kwon s.c.	1	108	22	252
labonté r.	3	191	62	1091
lahelma e.	2	740	39	1454
lampert t.	2	153	35	396
landrine h.	1	127	6	244
lang t.	2	70	27	451
laveist t.a.	1	774	46	1561
lawless a.	3	100	13	103
lee h.	1	143	23	354
levine r.s.	1	69	15	188
link b.g.	1	185	27	1471
lundberg o.	2	361	19	882
lundgreen a.	6	326	14	222
lynch j.w.	4	800	43	4648
macdougall c.	3	122	11	288
macintyre s.	2	90	8	953
mackenbach j.p.	2	919	77	2980
malmusi d.	3	250	23	457
manor o.	2	233	13	793
marmot m.	2	1268	106	10694
martikainen p.	2	530	39	954
matthews s.	2	218	6	867
mays v.m.	1	41	13	698
mckee m.	4	97	36	873
meade c.d.	6	87	24	233
mensah g.a.	1	66	22	1062
mielck a.	2	150	48	615
miller b.	8	148	12	1557
mitchell r.j.	7	295	26	1127
mohammed s.a.	1	241	9	1600
mokdad a.h.	1	48	6	1058
morello-frosch r.	1	127	12	892
morrison j.	3	190	17	368
muntaner c.	5	1058	80	1322
murray c.j.l.	4	157	17	2681
nazroo j.y.	2	213	31	1329
newman l.	3	123	11	117
ng e.	5	348	14	220
norman p.	2	84	13	187
o'campo p.	5	313	32	402
osypuk t.l.	1	125	15	473

paskett e.d.	1	32	32	493
payne-sturges d.c.	1	104	10	443
pearce j.r.	7	469	41	1279
peek m.e.	1	126	20	770
petrie d.	4	127	11	101
petticrew m.	3	749	45	2134
pförtner t.-k.	2	126	12	112
phelan j.c.	1	138	12	1183
pikhart h.	3	182	12	155
platt s.	3	121	11	381
pons-vigués m.	3	147	5	58
popay j.	3	136	22	805
popham f.	2	242	22	878
popkin b.m.	1	68	12	1859
potvin l.	2	126	18	974
powe n.r.	1	32	6	1063
power c.	2	292	17	1336
quandt s.a.	5	281	48	1165
rahkonen o.	2	428	30	830
raphael d.	3	136	36	623
rathmann k.	2	265	14	90
regidor e.	4	252	32	445
rehkopf d.h.	1	349	23	1676
richardson e.a.	7	142	12	244
richter m.	2	536	43	1006
rodriguez-sanz m.	5	291	19	319
roux a.v.d.	1	171	26	1391
rust g.	1	79	25	421
sacker a.	2	364	24	1073
schaefer c.t.	1	89	10	339
schrecker t.	3	158	37	594
schulz a.j.	1	264	33	1444
sekine m.	2	193	9	200
shaw m.	7	193	9	821
sheiham a.	4	250	29	966
shipley m.j.	2	313	12	1268
shortt n.k.	7	135	11	228
siddiqi a.	1	209	22	294
siegrist j.	2	99	18	578
singh-manoux a.	2	110	5	706
slattery m.l.	6	441	28	340
smith g.d.	2	689	35	4974

smith k.e.	3	302	30	525
solar o.	5	227	22	390
soobader m.-j.	1	213	5	1436
stansfeld s.a.	2	323	16	3311
stern m.c.	6	276	15	170
stringhini s.	2	112	8	702
stronks k.	2	165	29	564
subramanian s.v.	1	707	64	4030
swan j.	8	88	5	1186
szanton s.l.	1	104	16	347
thomas s.	3	205	31	884
thorpe r.j.	1	665	57	885
torres-mejia g.	6	301	14	251
torsheim t.	2	138	8	729
trinh-shevrin c.	1	120	26	299
tsakos g.	4	199	18	350
tugwell p.	3	415	33	913
turrell g.	2	117	33	1622
ueffing e.	3	128	14	286
vadaparampil s.t.	6	138	18	243
van ourti t.	4	160	13	208
van oyen h.	4	122	13	286
vandoorslaer e.	4	356	27	2169
vanroelen c.	5	291	16	379
victora c.g.	4	129	20	1525
viswanath k.	1	111	36	981
vives a.	5	190	10	320
wagstaff a.	4	205	12	1102
waterman p.d.	1	543	24	2506
waters e.	3	199	20	1046
watt r.g.	4	222	26	854
welch v.	3	371	28	689
west p.	2	235	13	1021
whitehead m.	3	533	46	1854
williams d.	1	871	57	4941
williams g.	3	109	18	458
wolff r.k.	6	441	27	323
wolfson m.	2	88	6	660
woolf s.h.	1	92	8	518
wright k.	3	92	9	404
wright r.j.	1	109	23	769
wyatt s.b.	1	96	11	535

^ Within network *containing keyworks within the specified search string

APPENDIX E: INTERVIEW CONSENT FORM

Ms Taya Collyer
GLOBAL PUBLIC HEALTH UNIT,
Social Policy, School of Social & Political Science
University of Edinburgh
Chrystal Macmillan Building,
15a George Square, Edinburgh, EH8 9LD
Tel: +44 (0)7 393 489 969
E-mail: taya.collyer@ed.ac.uk

Consent form for interviews relating to the research project entitled:
'Disciplinary Training and Epistemological Tension in Health Inequalities and Disparities Research'

I (Taya Collyer) am currently undertaking a PhD research project at the University of Edinburgh. The project is exploring the impact of disciplinary training on the conduct of research about health inequalities and disparities. One element of this research involves undertaking interviews with academics who conduct research about health inequalities/disparities. In these interviews, I am asking people to share their experiences of working with researchers from other academic disciplines, their perceptions of research conducted outside their own discipline, and reflections on the impact of their own training.

In line with the University of Edinburgh School of Social and Political Science's ethical guidance, I need to ensure that all participants in this study have decided to participate, and are aware that they do so voluntarily and are not required to participate. If you are willing to participate in this study, please sign this form (over-page) to confirm that you have freely agreed to be interviewed.

If you do agree to be interviewed, this does not mean you need to answer any or all of the questions. You may cancel or stop the interview at any time, simply by letting me know that you wish to do so.

If you agree, the interview will be digitally-recorded and subsequently transcribed by me. Personal details will be anonymised (unless you elect to remain identified, see options over page), and I will not intentionally reveal your identity to anyone without your written consent. If you request to do so, you will be given a chance to review and amend the transcript for accuracy and/or anonymity in due course (see options over page).

Once anonymised, the contents of the interviews – including yours – will be analysed and written up during the course of the research. The findings may be used in published works, such as academic journal articles or scholarly texts. This written work may include quotations from some of the interviews, including yours. If you wish to remain anonymous, neither your own name, nor that of your organisation, or any of your other personal details that might identify you will ever be associated with these quotations. Alternatively, if you expressly wish your name to accompany your comments in published findings, please indicate this below.

I would be grateful if you could confirm, by signing the form below, that you are happy for me to use the recorded interview or extracts from it in this way.

– – –

CONSENT FORM

I confirm I have freely agreed to be interviewed for this project and that the recorded interview or extracts from it may be used as described above.

PLEASE STRIKE THROUGH PART OF THE STATEMENTS BELOW ACCORDING TO YOUR PREFERENCE:

I would like / would not like to be sent the interview transcript in order to allow me to check it for accuracy and anonymity prior to any sections of the transcript being used in publications.

I would like / would not like to be identified by name, should any sections of the interview transcript be included in publications.

(NB If you request to see the transcript, please provide Taya with new contact details if your details change.)

Signed:
………………………………………………………………………………………………

Name:
………………………………………………………………………………………………

Date:
………………………………………………………………………………………………

APPENDIX F: CODES USED FOR ANALYSIS

[1] "Health, what is"	"Why interested in health"
[3] "Journeys, pragmatic"	"Health, Mental Models"
[5] "Geography"	"Social Science, Strength"
[7] "Biomedical Model"	"Integrating different levels"
[9] "Policy"	"Politics"
[11] "HIDR - ideas abt \"HI researchers\""	"Economists"
[13] "Extent of interdisc. collab"	"Interdisc work - communicating/terminology"
[15] "Interdisc work - positive exp"	"Interdisc work - challenges"
[17] "Determinants of interdisc. success"	"Interdisc. work - negative exp"
[19] "Interdisc work - compromises"	"Interventions, research abt"
[21] "Good empirical work"	"Seeing the big picture"
[23] "Activity Ranking"	"Sample Size"
[25] "Qual work, positive"	"Feedback on Interview"
[27] "Statistics - Own use of"	"Models - Discovery or Construction"
[29] "Truth / Reality"	"Statistics - Self deprecating remarks"
[31] "HIDR - tips for ECRs"	"Descriptive Work - Limits of"
[33] "Causality -Debate"	"USA cf UK"
[35] "Disciplinary Identity"	"USA - Terminology"
[37] "My job..."	"Journeys, seeking impact"
[39] "Journeys, close to patients"	"ECRs"
[41] "Study Design"	"Science, what is"
[43] "Statistics - Underpowered studies"	"Statistical Literacy"
[45] "HIDR - Challenges"	"Epidemiologists"
[47] "Journals"	"Interdisc work - its the individual"
[49] "Frustrating research - Unimportant Questions"	"Values and science"
[51] "Journeys, other"	"Model Building"
[53] "Frustrating research"	"Public health is..."
[55] "Epi Paradigm / Ev Pyramid"	"Getting into the body"
[57] "Qual work, negative / mixed"	"Data Access"
[59] "Causality"	"Phacking/Fishing"
[61] "P-Values"	"Models, what do"
[63] "Direction for future"	"Statistics - trends over time"
[65] "Statistics - Linear regression"	"Academic training"
[67] "Theory, role of"	"Medical Sociology"
[69] "Social Epidemiology"	"Journeys, challenges"
[71] "Feelings about home discipline"	"Focus is the questions"
[73] "Disciplinary differences on same concept"	"Shadow of the paradigm"
[75] "Two Worlds"	"HIDR - Important Questions / Debates"
[77] "Inscription device"	"Epidemiology"
[79] "Genetics"	"IQ and Health"
[81] "RCTs"	"Interdisc work - do you?"
[83] "Publishing"	"Interdisc work - how to succeed"
[85] "Disciplines"	"Epidemiology - Subfields"

[87] "MDs" "Mendelian Randomisation"
[89] "Isolation from home disc" "Sociology"
[91] "Biologists" "Model, ultimate"
[93] "Sociologists" "Statisticians"
[95] "HIDR - Other" "Literature Appearing"
[97] "Population, idea of" "Economics"
[99] "Disciplinary differences on same method" "Advocacy/Activism"
[101] "Descriptive work - Strengths of" "Public Health People/Types"
[103] "For Revisit" "Statistics - Other"
[105] "Contrast" "Interdisc Work - Good collaborators"
[107] "Statistics - Good Statistical collaborator" "Funding"
[109] "Social Sciences" "Mechanism"
[111] "Models - problematic that there are several?" "Data"
[113] "Interesting" "Measurement"
[115] "Methods belong to disc" "Interdisc work - incentives and motivations"
[117] "Countries" "Unexpected results"
[119] "Simplification vs complexity" "Assumptions about me"
[121] "Creativity" "Psychology"
[123] "Literature - (purposive knowledge)" "Intersections"
[125] "Similarities & Common Ground" "UK cf Europe"
[127] "Evidence" "Critique of HIDR frameworks"
[129] "Interdisc work - is one interdisc person" "References or quotations"
[131] "Health Behaviours" "Disciplinary Hierarchy"
[133] "Natural Experiments" "One Idea"
[135] "Context" "Key Questions"
[137] "Disease in Individuals" "Neoliberalism"
[139] "CPBR type stuff" "Race"
[141] "Scientist, what is" "Academia"
[143] "Interesting Language" "Agent Based Models"
[145] "Mixing Methods" "Anthropology"
[147] "generalisability" "Being Unsure what others think"
[149] "Evidence Based something" "Risk"
[151] "Specialisation" "HIDR - Network comments"
[153] "Good work is hard" "Statistics - Across a career"
[155] "Surgery" "Disease focus"
[157] "Literature - Classification" "Literature - Ontology"
[159] "Regression equation language" "Literature - Words of Nature"
[161] "Literature - Epistemology" "Literature - Small Lifeworlds"
[163] "Language" "Political Science"
[165] "critical perspective" "Time/Temporal issues"
[167] "Balancing theory and method"

Appendix G: Sample Analysis Spreadsheet

		THEORY IS	
		In public health, theory = hypothesis	XXXXX
		Not a hypothesis	XXXXX, XXXXX
"chain of causal pathways" XXXXX	"causal model"	Theory is in the DAG	XXXXX, XXXXX, XXXXX, XXXXX
		Classical Sociology	XXXXX, XXXXX
		"real world theories"	XXXXX
		Medicine is theory	XXXXX, XXXXX

Social Theory	LANGUAGE	Conceptual Framework'	Causal Framework/ Model/DAG
XXXXX, XXXXX, XXXXX		<XXXXX, <XXXXX, XXXXX>	XXXXX, XXXXX
XXXXX, XXXXX, XXXXX, XXXXX, XXXXX	Social theory should underpin model		

	THEORY DOES		
XXXXX, XXXXX (coherence)	Stops you getting lost in data	Can Be amazing, 'lightbulb'	XXXXX, XXXXX
XXXXX, XXXXX, XXXXX, XXXXX, XXXXX	Guides Qn, Method, Ax,	Models of social change	XXXXX, XXXXX
XXXXX	Most important part of model dev	What do findings "mean"	XXXXX, XXXXX, XXXXX
XXXXX, XXXXX, XXXXX, XXXXX	Guides variable selection	Good work tests theory	XXXXX, XXXXX, XXXXX
XXXXX	Doesn't help (purposive)	BUT - not everyone is open to theory being tested.	XXXXX, XXXXX (see 'public health types"
XXXXX/XXXXX	Ambivalent/impatient with theory	Theory ain't Theory - can be shallow or just a structure for findings	XXXXX

		DISCIPLINES	
XXXXX, XXXXX	Getting methodologists to to take theory seriously is hard	Different disciplines understand theory differently	Neighbourhoods :(
XXXXX, XXXXX	"true" social scientist, theory frames everything	Even Social Epi isn't enough	XXXXX
XXXXX, XXXXX, XXXXX, XXXXX, XXXXX, XXXXX	EPI is weak in theory	Public health isn't atheoretical	XXXXX
XXXXX	Lack of theory in epi shapes qns	Psych has theory on individual behaviour	XXXXX
XXXXX, XXXXX	Medicine is theory	In Geo, develop rather than test theory	XXXXX? XXXXX?
XXXXX	Developing theory is a waste of time	Geo rich in social theory	XXXXX

Training & Academia	
Epi Journals don't have space for theory	XXXXX
Hard to publish theoretically informed work	XXXXX, XXXXX, XXXXX
Squeeze in academia reduces space for theoretical next steps	XXXXX
Theoretically aware quant work is hard to train for	XXXXX

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An atlas of health inequalities and health disparities research: "How is this all getting done in silos, and why?"

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ABSTRACT

Research on health inequalities and health disparities has grown exponentially since the 1960s, but this expansion has not been matched by an associated sense of progress. Criticisms include claims that too much research addresses well-trodden questions and that the field has failed to gain public and policy traction. Qualitative studies have found researchers partly attribute these challenges to fragmentation resulting from disciplinary and methodological differences. Yet, empirical investigation ('research on research') is limited. This study addresses this gap, employing mixed-methods to examine, at scale, how and why this field is defined by insular research clusters. First, bibliometric analysis identifies and visualizes the 250 most-connected authors. Next, an algorithm was used to identify clustering via citation links between authors. We used researcher profiling to ascertain authors' geographical and institutional locations and disciplinary training, examining how this mapped onto clusters. Finally, causes of siloing were investigated via semi-structured interviews with 45 researchers. The resulting 'atlas' of health inequalities and health disparities research identifies eight clusters of authors with varying degrees of connectedness. No single factor neatly describes observed fragmentation, health equity scholars exhibit a diverse disciplinary backgrounds, and geographical, institutional, and historical factors appear to intersect to explain siloed citation patterns. While the configuration of research activity within clusters potentially helps render questions scientifically manageable, it affirms perceptions of the field as fragmented. We draw on Thomas Kuhn and Sheila Jasanoff to position results within theoretical pictures of scientific progress. Newcomers to the field can use our findings to orient themselves within the many streams of health equity scholarship, and existing health equity scholars can use the atlas to move beyond existing geo-disciplinary networks. However, although stronger cross-cluster engagement would be likely to improve insights, the complex nexus of factors underlying the field's structure will likely make this challenging in practice.

1. Introduction

Health inequalities research and health disparities research (hereafter 'health equity research') aims to understand, explain and reduce the unequal distribution of health across groups defined by social demographic factors, such as education, income and ethnicity. Previous studies chart the exponential accumulation of articles on this topic from 1966 onwards, demonstrating wide geographical interest, though a dominance of US and UK contributions (Bouchard et al., 2015; Cash-Gibson et al., 2018). However, growth in research has not been matched by improvement in health equity. Many countries have charted persistent health gaps between their most and least marginalized groups (Pool et al., 2017; Smith et al., 2015). Although slightly more positive accounts have recently emerged from some Western European countries (Mackenbach et al., 2018), researchers have nonetheless been critical of the field's lack of success. While some of these critiques emphasize the failure of political and policy actors to respond to available evidence (McCartney et al., 2013; Qureshi, 2013), there have been at least three sets of charges levied at researchers.

The first relates to failure to adequately translate research into policy and practice (often directed jointly at researchers and policymakers). Here, for example, Lynch argued scholars and policymakers employ health inequalities as a medicalized frame for discussing social inequalities and, in so doing, made the issue politically more appealing but

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'technically quite difficult to solve' (Lynch, 2017). Multiple authors highlight the lack of coalitions connecting health equity scholars to those with advocacy, media and policy expertise (Smith et al., 2015; Bambra et al., 2011). Other critiques focus on researchers' perceived failure to engage with the public, noting that perceived public preferences often inform what is deemed politically feasible (Garthwaite et al., 2016; Smith, 2013).

The second set of criticisms relate to research content. Here, charges include a tendency to investigate well-trodden questions (e.g. describing patterns and causal pathways), while neglecting the impacts of policies and interventions (Bambra et al., 2011; Smith, 2013; Garthwaite et al., 2016). For example, Bambra and colleagues criticize researchers and policymakers for failing to collaborate in ways that enable the effects of policy changes on health inequalities to be adequately assessed (Bambra et al., 2011). While a survey of health inequalities researchers working in the UK suggests researchers themselves feel the field has been preoccupied with downstream risk-factors, such as smoking, whilst producing insufficient research on upstream, structural and social determinants (Smith and Eltanani, 2014). This imbalance has, in turn, been linked to researchers' perceptions of what is likely to be funded (Smith, 2010), suggesting research funders also shape the field.

The third set of issues relate to perceived lack of connectivity. Division is evident even in the characterization of fundamental terms and concepts. As Bouchard et al.'s (2015) bibliometric analysis demonstrates, the terms 'health inequities', 'health inequalities' and 'health disparities' are used inconsistently (sometimes interchangeably), suggesting the presence of distinct scientific - perhaps epistemic - communities. The boundaries separating these communities have, in turn, been linked to geographical norms (Graham, 2004), disciplinary preferences (Bouchard et al., 2015) and 'issue framing' (Gamble and Stone, 2006). The latter dimension appears particularly confused; while there is some consensus that the term 'health disparities' is descriptive, some claim 'health inequalities' better signals health differences as unfair and unjust (Gamble and Stone, 2006), while others claim that only 'health inequities' is imbued with this moral dimension (Ward et al., 2013; Braveman, 2006).

Beyond differences in terminology, qualitative research suggests disciplinary divisions are particularly important. Bartley's analysis of research on unemployment and health in the UK highlighted the importance of differences between economics, statistics and sociology, noting that these cut across the boundary between research and policy (Bartley, 1992). More recently, Wade and Stone's (2010) reflections on teaching health disparities note that sociology and economics tend to approach this issue with entirely different questions and assumptions. Garthwaite and colleagues' (2016) study exploring how researchers feel the field should progress identified three epistemological clusters, each supporting distinct ways forward.

These three sets of critique are interrelated, with the first two being at least partially connected to the third. For example, fragmentation arising from geographical and disciplinary differences has been connected to claims that the field lacks clear advocacy-coalitions (Smith, 2013). While methodological preferences of dominant disciplines (especially epidemiology), and the lack of interdisciplinary collaboration, have been charged with contributing to the field's ongoing production of 'partial investigations' (Garthwaite et al., 2016). In other words, a belief that health equity research is fragmented is viewed as problematic for the field in multiple ways.

Understanding how and why health equity research is siloed therefore seems important. Yet there has been little empirical examination of the field (sometimes referred to as 'research on research') and even less examining disciplinary diversity. This study helps address this gap, employing mixed methods to examine, at scale, how the field is organised within citation-space and to ascertain the roles of disciplinary, geographical and institutional factors in the establishment and maintenance of this observed structure. By employing a bibliometric approach, we provide a much broader view of divisions within the field than qualitative studies published to date. We also move beyond existing bibliometric analyses by analyzing the connectivity of authors, not papers. Our analysis provides fresh empirical support for claims that the field is fragmented and, for the first time, identifies notable clusters of authors within the field and their relationship to one another in citation-space. Researcher profiling and qualitative interviews with researchers featuring in the bibliometric analysis are then used to shed light on these findings, demonstrating that disciplines, institutions and geographies have intersected to contribute to the emergence of distinct clusters (or 'silos'). These findings can help newcomers to the field orient themselves within the literature, and also have important implications for those seeking to promote interdisciplinary collaboration within health equity research.

2. Methods

To avoid reliance on low-quality institutional affiliation data (Rafols and Meyer, 2007) or a focus on highly-cited researchers, bibliometric analysis was employed to identify the 250 most-connected authors within health equity research. This number was felt to be visually manageable, while including a range of career stages and locations.

We selected Scopus, the largest academic database (Mongeon and Paul-Hus, 2016) as the source database and article titles, abstracts and keywords were searched using the search string below, to extract records dated between 1976 and 2016. No geographical or language restrictions were applied. However, the use of English search-terms restricted results to those with an English title, abstract and/or keywords.

(("health inequ*") OR ("health equal*") OR ("health equit*") OR ("health disparit*") OR ("social determin* of health"))

As bibliometric analysis at the author level requires a high standard of data-hygiene (Wagstaff and Culyer, 2012), records were read into an SQL database for cleaning (correcting misspellings, merging authors appearing under multiple names, and distinguishing authors sharing names).

The bibliometric analysis aimed to visualise health equity research and to uncover patterns in citation flows that enhance understanding of the extent to which members of the field are integrated or segregated within disciplinary, geographic or institutional silos. Authors with five or more publications meeting search criteria were eligible for inclusion, and distances between author pairs calculated via direct citation (how many times Author A cites Author B). This method was selected over analysis of co-authorship as, when contrasted with article authorship, citations more completely reflect the material on which scholars draw and the literatures to which they feel their work connects.

A clustering algorithm was then employed to highlight regions of the network where a high proportion of citations are local. Waltman & Van Eck's Smart Local Moving algorithm (2013), was used, which initialises by assigning each author to a cluster of which they are the only member (i.e. 250 clusters where n = 1), and moves authors between clusters until the proportion of citations within groupings is maximised. Therefore, an author may cite authors from any cluster, but they tend to be cited (and/or to cite) authors from their own cluster.

Next, we used what we term 'researcher profiling' to ascertain the institutional and geographical location of authors, as well as their disciplinary training. To gather this information we undertook online searches and, where necessary, contacted researchers directly via email. Since observed diversity in disciplinary training could arise due to variation in course titles between countries, institutions, or over time, we classified researchers' highest degree (e.g. PhD) into Subject Categories, using Porter and Rafols' (2009) mapping. Following Jost (2007), the Shannon Number Equivalent (SNE) index is presented as a measure of disciplinary diversity within clusters. This can be interpreted as the number of equally-represented disciplines required to achieve the observed diversity of each cluster. Bibliometric analyses were conducted using VOSviewer (Van Eck and Waltman, 2017) and diversity measures calculated using

Stata (Statacorp).

In addition, the first author undertook semi-structured interviews with 45 researchers appearing in the network, to better understand their disciplinary backgrounds, career development and views about health inequalities/disparities research. Interviewees were shown an early version of Fig. 1, enabling them to reflect on clustering. Interviews were conducted using a themed interview schedule, via in-person meetings (n = 15), phone-calls (n = 3) and video-calls (n = 28). 113 researchers were invited to participate, and, with the exception of Cluster 5 (from which no interviews resulted), at least 3 representatives from all clusters were interviewed. Interviews were recorded and transcribed verbatim by the first author and thematically coded using the R Package RQDA (Huang, 2016) to aid analysis. This research project, including interviews, received ethical approval from the University of Edinburgh School of Social and Political Science.

This study does not present the entire, global health equity research network, but the 250 most densely-connected set of authors, meaning that satellite communities not citing any author in Fig. 1 are excluded. Our long time-scale may introduce bias toward late-career, established researchers. However, as over 80% of individual papers analysed were published after 2008, this impact seems likely to be minimal. Finally, as researchers' PhD discipline may not accurately reflect the discipline of their current research output, some disciplinary communities may remain undetected.

3. Results

We begin by providing an overarching summary of the visual network produced via the bibliometric analysis, before exploring geographical and disciplinary dimensions of the network. Together with the researcher profiling, 45 interviews and our analysis of the cited papers, this informs the subsequent brief descriptive account of the eight clusters within the network. The final section of the results then draws directly on the interview data in attempt to understand this clustering in more depth.

3.1. Network overview

29,212 papers containing relevant keywords were extracted, representing over 8500 authors. Citation flows between pairs of authors were analysed, the 250 most-connected authors identified, and arranged such that authors with strong citation links are located close together, producing Fig. 1. Hence, Fig. 1 depicts the 250 most-connected authors publishing research in English about health equity between 1976 and 2016 (hereafter, "researchers" or "authors"). Despite spanning 40 years, 25,165 of papers (86%) were published after 2008, reflecting the exponential increase noted by existing reviews (Cash-Gibson et al., 2018; Bouchard et al., 2015).

Fig. 2 is a simplified version of Fig. 1, highlighting the eight clusters. The left and right halves of Figs. 1 and 2 have different spatial arrangements and citation structures; the left is made up of five densely-connected, partly-overlapping clusters (labelled 1, 2, 6, 7 and 4 in Figs. 1 and 2); while the right comprises three non-overlapping clusters with relatively sparse interconnectivity (labelled 5, 8 and 3). Generally (though with exceptions), researchers from the US appear on the right hand side of Fig. 1 and researchers from the UK, Europe, Australia, Canada and Europe appear on the left. In the following sections we investigate authors' geographic distribution, and other author attributes which seem to overlay and intersect with geography to produce the structure of Fig. 1.

3.2. Network geography

The geographic location of network members is summarised in Table 1. Reflecting the findings of studies focused on the geographical spread of the field (Bouchard et al., 2015; Cash-Gibson et al., 2018), two-thirds of researchers are based in the US or UK, and an additional 13% in Canada and Australia. The remainder of the network comprises researchers from Europe, Scandinavia, Latin America and South-East Asia.

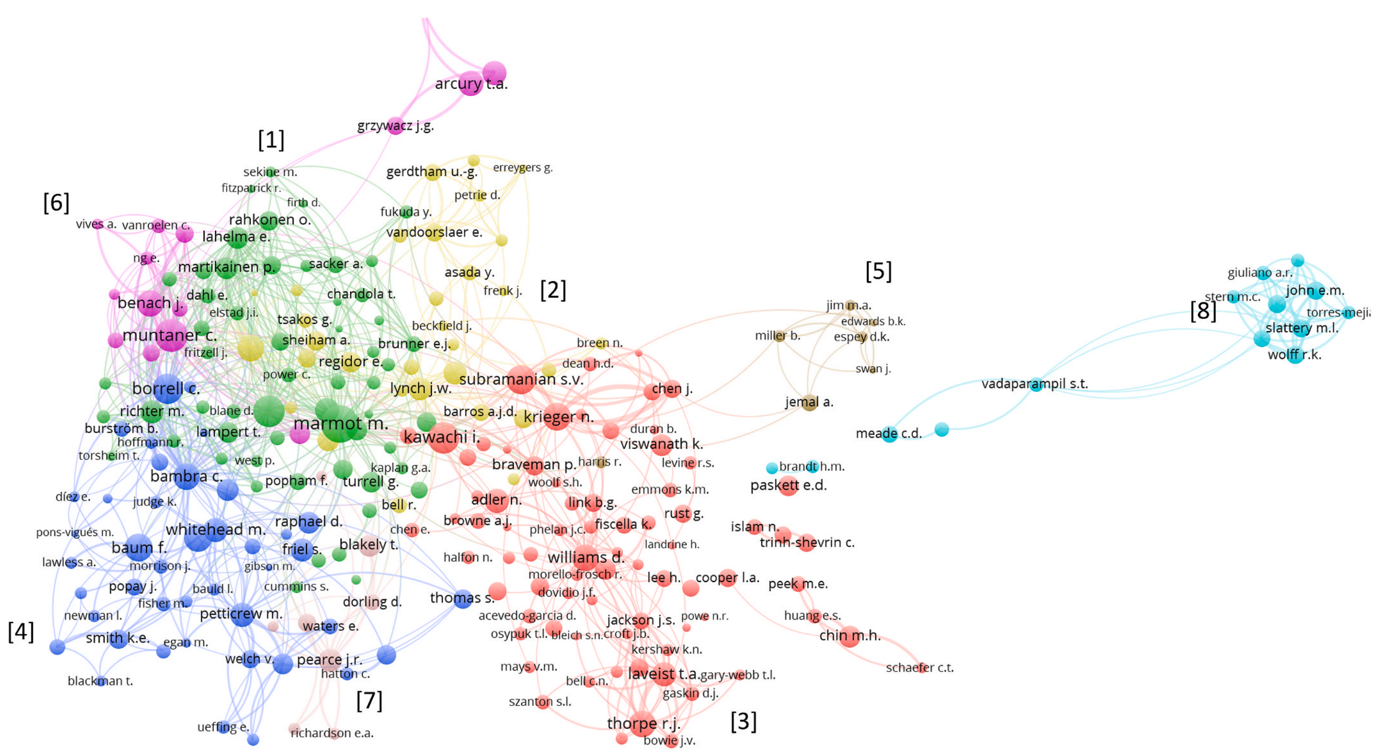

Fig. 1. The 250 most-connected health equity researchers. Nodes represent authors who have published at least 5 papers with relevant keywords. The size of the node/circle represents the number of papers each author has published. Width of lines indicates the number of citations between authors. The colour of the nodes represent different clusters (numbered 1–8) of authors detected via algorithm.

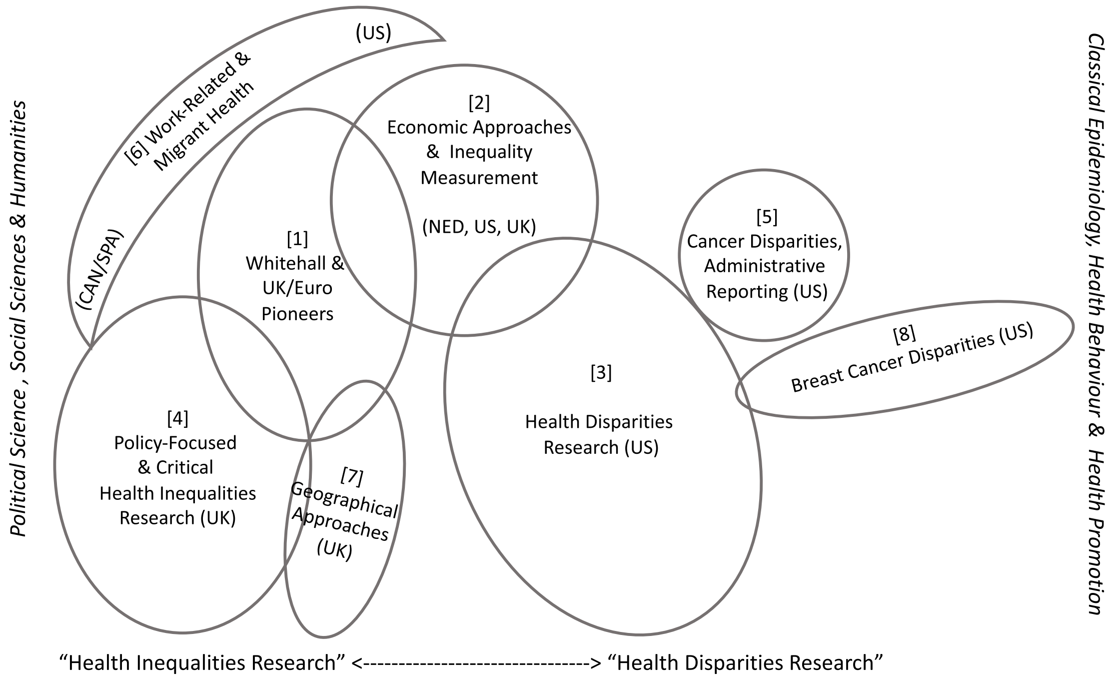

Fig. 2. Eight clusters of health equity research.

3.3. Network disciplines

Table 2 provides a breakdown of first degree and highest degree by subject category, providing the first detailed picture of the disciplinary backgrounds underpinning the health equity research field. A wide range of disciplines are represented in network members' first degrees. Perhaps unsurprisingly, there is less variation in researchers' PhD or other highest degree (Juris Doctor, DPhil, etc), with the most common category being 'Public, Environmental & occupational health' (which includes epidemiology, health promotion, health behaviour and health education). This is followed by sociology, medicine, psychology, economics/health economics, and political science. Almost one in five network members have a medical qualification.

Such varied undergraduate training suggests the field potentially incorporates diverse ideas about health, and health equity. However, the most common ideas are likely to be shaped by the dominant disciplines which are, in order, public health, sociology, medicine, psychology, economics, and political science.

Table 1
Network members' geographical location.

Country	Count	%
US	108	43%
UK	59	24%
Canada	19	8%
Australia	12	5%
Netherlands	8	3%
Germany	7	3%
Spain	7	3%
Sweden	5	2%
Brazil	3	1%
Finland	3	1%
Norway	3	1%
Belgium	2	1%
Chile	2	1%
Japan	2	1%
South Korea	2	1%
New Zealand	2	1%
Switzerland	2	1%
Other	4	2%
Grand Total	250	100%

In the following section we explore the clusters in Figs. 1 and 2, including via examination of disciplinary diversity.

3.4. Network clusters

Fig. 1 contains the set of 250 most-connected health equity scholars, in terms of citation links. A large volume of citations flow across cluster boundaries, however the eight clusters visualised represent network regions where a high proportion of citations are local (i.e., within-cluster). Combining demographic data with the citation network allows investigation of the extent to which disciplinary diversity is uniform across Fig. 1, or maps onto the clusters (suggesting disciplinary silos). Fig. 2 is a simplified version of Fig. 1, highlighting the arrangement and general character of the eight clusters. The labels in Fig. 2 reflect analysis presented in this section, which considers what research profiling and interviews revealed about the intersecting influence of disciplines, geography, history, and research focus on cluster formation.

Table 3 contains key details regarding each cluster, including size, proportion in the US/UK, proportion with a medical qualification, and members' highest degree by subject category. The US/UK breakdown is presented as while these countries account for over 50% of the network, they are not equally represented within any cluster, and some clusters are dominated by either the US or UK.

Table 3 also reports the SNE index, reflecting both the number of disciplines and evenness of their representation within clusters. The entire network of 250 authors has SNE of 14.15, equivalent to approximately 14 equally-represented disciplines (or, 250/14–18 examples of each discipline). Only Cluster 4 is as diverse as the wider network, suggesting a degree of disciplinary sorting or concentration. This possibility is explored below, as we examine each cluster in turn, in order of increasing median year of first publication.

Cluster 1: Whitehall Investigators & Health Inequalities Pioneers (UK/Europe).

The network's oldest cluster (by median entry to the field) is comprised largely of researchers from the UK and Europe who began studying health inequalities during the 1980s and 1990s. The relatively high diversity score reflects the wide-ranging backgrounds of these early inequality scholars, which included many social scientists. This cluster is especially notable for its high proportion of Sociology PhDs, making up

Table 2
First Degree and PhD/Highest Degree by Subject Category Data obtained from CVs, online profiles, and (where necessary) from researchers directly via email.

Subject Category	First Degree	PhD (Or Highest Degree)
Public, environmental & occupational health	6	74
Sociology	27	32
Medicine, general & internal	44	22
Psychology	26	13
Economics	11	12
Political Science	8	11
Geography	12	8
Social sciences, biomedical	3	8
Statistics & probability	3	8
Psychology, clinical		7
Nursing	8	5
Demography		4
Health policy & services		4
Biochemistry & molecular biology	2	3
Health care sciences & services	8	3
Medicine, research & experimental	2	3
Social sciences, interdisciplinary	7	3
Anthropology	2	2
Behavioral sciences		2
Dentistry, oral surgery & medicine	3	2
Ecology	1	2
History	7	2
Social work	2	2
Urban Studies		2
Biology	15	1
Business		1
Communication		1
Education & educational research		1
English/Literature	7	1
Family studies		1
Genetics & heredity	1	1
Information science & library science	1	1
Law		1
Nutrition & dietetics		1
Philosophy		1
Planning & development		1
Chemistry	6	
Design	1	
Engineering	2	
Management	1	
Mathematics	5	
Microbiology	2	
Multidisciplinary sciences	3	
Neurosciences	1	
Pharmacology & pharmacy	2	
Public administration	3	
Religious Studies	2	
Veterinary sciences	1	
Zoology	2	
Unknown	13	4
Total	250	250

just over a quarter of the cluster, while psychology, the social sciences and political science are also represented. The upper left part this of cluster includes several sociologists from Scandinavia and Germany. Researchers toward the bottom of this cluster (overlapping Cluster 7) share a focus on place and health.

Cluster 2: Economic approaches & Measurement of income inequity at scale (Netherlands, UK, US).

Cluster 2 is comprised of two distinct regions, each with disciplinary features. Work in this cluster emerged in the early 2000s and addresses methodological issues arising from the international scaling-up of studies like those occurring in Cluster 1 (discussed in Section 3.4). Located at the top are economists from the Netherlands, Australia, Sweden, and the US, who have contributed advances in the measurement of health equity. Near the centre of the network are a group of epidemiologists and social epidemiologists, many of whom have medical or sociology backgrounds. Both regions share an interest in the *measurement* of health equity, and the ways in which money, income and prevailing economic conditions affect health, including dental and oral health. Cluster 2 is around three-quarters the size of Cluster 3, but demonstrates 35% of that cluster's diversity, reflecting the smaller number of disciplines represented.

Cluster 3: Health Disparities Research (US). Almost all (96%) of the network's largest cluster is located within the US, and half hold a PhD or other doctoral degree in Public health (including epidemiology) or medicine. Nursing, psychology and sociology PhDs are also represented, but political science and the humanities are absent. While the majority of authors in Cluster 3 have written about ethnic and racial disparities in health, researchers in the rightmost region of this cluster (near clusters 5 and 8) share a focus on racial and ethnic disparities in cancer outcomes. Researchers in the leftmost part of the cluster have a more mixed focus, including, for example, maternal and child health, drug use, mental health, and allergies. The top-left corner includes several researchers from the Harvard School of Public Health, and University of California San Francisco. These researchers have focused on the relationship between socioeconomic status and health, perhaps explaining their strong citation links with Clusters 1 and 2. The emergence of Cluster 3, and its links with Clusters 5 and 8, are discussed in Section 3.4.

Cluster 4: Policy-Focused & Critical Health Inequalities Research (UK).

Cluster 4 is unique within the network for its disciplinary diversity. The US is absent from this cluster, with 52% of members located within the UK, 19% in Australia and 17% in Canada. While the median entry to the field for authors was 2005, Cluster 4 generally developed alongside Cluster 1, throughout the 1980s and 1990s, as the strong citation links in Fig. 1 indicate. Members of this cluster have the network's most-diverse doctoral training, with an equivalent 13 equally-represented disciplines among just 42 members. The network's humanities PhDs and political science PhDs are concentrated here, and the social sciences are also well-represented. These trainings are consistent with the cluster's focus on macro or 'upstream' determinants, including political and corporate determinants. Many researchers in this cluster conduct qualitative research and have a theoretical emphasis in their work. The cluster covers topics such as health policy, lay knowledge and evidence synthesis. Together, these features reflect the conceptualisation of health within Cluster 4 as socially-situated. This cluster has strong citation links to Clusters 1, 7 and 6, but is sparsely linked with Cluster 3.

Cluster 5: Racial and Ethnic Disparities in Cancer: Administrative Reporting (US).

Cluster 5 is the network's smallest cluster and (like Cluster 8) is cancer-focused and located geographically within the US. Members of this cluster have co-authored highly-cited, national administrative cancer statistics reports which include cancer incidence and mortality for racial and ethnic subgroups (labelled with the keyword "health disparities" since 2002). Nearly one third of members have medical training, and remaining members have statistical, biomedical, or public health backgrounds, reflecting the output of this cluster. Most members are affiliated with either the National Cancer Institute, National Cancer Society, Centers for Disease Control, or National Institutes of Health (NIH).

Cluster 6: Socio-Critical accounts of Work-Related and Migrant Health (US, Canada, Spain).

Cluster 6 is a mix of researchers from Europe, Latin America and the US. Researchers in this cluster are linked via a joint focus on employment-related health disparities/inequalities, and migrant health. Several researchers are based in Barcelona or completed doctoral study in that city. An additional group are located at (or have passed through) the Wake Forest Department of Family Medicine, in the US. In terms of disciplinary training this is the most diverse of the small clusters (Clusters 5–8), containing a mix of researchers with medical, biomedical, family studies, health policy, psychology, and statistical backgrounds. The network's anthropology PhDs are also concentrated within this cluster, and this similar disciplinary profile perhaps explains the position in citation-space alongside Clusters 1 and 4, within which

Table 3
Network Cluster characteristics.

Cluster	1	2	3	4	5	6	7	8	Total
(n)	57	31	76	42	7	15	8	14	250
Number Equivalent Shannon Index	9.68	4.53	9.68	13.07	3.6	8.33	4.01	3.42	14.15
% US	7%	19%	96%	0%	86%	26%	0%	93%	43%
% UK	40%	19%	1%	52%	0%	0%	88%	0%	24%
% Any Medical Degree	18%	32%	20%	12%	29%	40%	13%	7%	20%
Median year of authors' first included publication	1999	2002	2004	2005	2005	2006	2008.5	2011.5	2004
Earliest first included publication	1985	1997	1993	1983	2002	1999	2001	2003	1983
Subject Category: PhD/Highest Degree			**(% of cluster)**						**(% network)**
Public, Environmental & Occupational Health	19%	45%	36%	19%	43%	20%	0%	64%	30.00%
Medicine, general & internal	4%	6%	14%	5%	29%	13%	13%		8.80%
Medicine, research & experimental					14%	13%			1.20%
Nursing			5%					7%	2.00%
Dentistry, oral surgery & medicine		3%	1%						0.80%
Nutrition & dietetics			1%						0.40%
Psychology	11%		5%	5%		7%			5.20%
Psychology, clinical			8%	2%					2.80%
Behavioral sciences			3%						0.80%
Education & educational research				2%					0.40%
Statistics & probability	7%		1%		14%	7%		7%	3.20%
Biochemistry & molecular biology	4%							7%	1.20%
Ecology	2%						13%		0.80%
Biology								7%	0.40%
Genetics & heredity								7%	0.40%
Health policy & services			3%			13%			1.60%
Health care sciences & services	2%			5%					1.20%
Planning & development				2%					0.40%
Family studies						7%			0.40%
Demography	4%	3%		2%					1.60%
Urban studies	2%			2%					0.80%
Geography	2%		1%	5%			50%		3.20%
Communication			1%						0.40%
Business			1%						0.40%
Economics	2%	29%	3%						4.80%
Social sciences, biomedical	4%		1%	7%			13%		2.80%
Social sciences, interdisciplinary	2%			2%		7%			1.20%
Social work	2%		1%						0.80%
Sociology	26%	6%	9%	17%			13%		12.40%
Political science	4%	3%	3%	14%					4.40%
Anthropology						13%			0.80%
Law			1%						0.40%
History		3%		2%					0.80%
Literature				2%					0.40%
Philosophy				2%					0.40%
Information science & library science				2%					0.40%
Unknown	7%								2.00%
Total	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100%**	**100.00%**

health is characterised as a socio-structural phenomenon.

Cluster 7: Geographical Approaches: Inequalities in Place & Space (UK).

This relatively new, chiefly UK-based cluster contains the majority of the network's geography PhDs. Located at the right-hand margin of Cluster 4, these researchers share a focus on spatial and geographic inequalities, environmental justice, and neighbourhoods. These eight researchers entered the field slightly later than researchers in other clusters, with a median first publication year of mid-2008.

Cluster 8: Breast Cancer Disparities (US).

This small cluster is comprised chiefly of contributors to a single project, the Breast Cancer Health Disparities study (Slattery et al., 2014), many of whom are (or were once) based at the University of Louisville, Kentucky, or University of Utah. This is the least-diverse cluster in terms of disciplinary background, with a concentration of researchers holding a PhD in Health Behaviour, Health Education or Health Promotion (consolidated within the Public Health Subject Category). Other members hold advanced degrees in statistics or biomedical science. This is the network's most recently established cluster, with members' median first publication in the field being mid-2011.

3.5. Exploring and explaining the eight research clusters

In this section, we draw on interview data to better understand how clusters have emerged and by what forces they are sustained in citation-space. As a reminder, we interviewed 45 network members and incorporated questions about an early version of Fig. 1. Although we use extracts illustrate specific points, we also draw on data collectively to draw out common explanatory narratives that appear to explain the evolution of the field and the clustering identified above.

3.5.1. Landmark studies & advances in measurement

Major research projects have contributed to the form and disciplinary topology of Fig. 1. An early milestone of health inequalities research was the Whitehall cohort, established in 1967 and analysed from 1978 onwards to investigate the relationship between cardiovascular (and other) diseases and occupational social class within the British civil service (e.g. Rose and Marmot, 1981). The status of the Whitehall studies as paradigmatic examples of health equity scholarship is reflected in Fig. 1, as Whitehall investigators and collaborators make up much of Cluster 1, and occupy a central position in the network.

Dutch researcher Johan Mackenbach spearheaded efforts to replicate Whitehall in Europe, beginning with the Dutch Longitudinal Study on

Socio-Economic Health Differences, containing an explicit reference to Whitehall in its abstract (Mackenbach et al., 1994). Authors of similar single-country studies make up the top (left) half of Cluster 1. These replications provided comparable cohorts in several high-income countries and, therefore, the opportunity for cross-country comparison. This work was initiated by a group of economists, visible in Cluster 2. Throughout the late 1990s and early 2000s, the scaling-up of the field from single cohort studies to global mega-comparisons introduced methodological challenges, and an accompanying need for "valid measures and methods" (Manor et al., 1997). In response, a literature specific to the measurement of health equity emerged, largely authored by the economists in Cluster 2 and others located at the intersection of Clusters 1, 2 and 3 (Kakwani et al., 1997; Kawachi and Kennedy, 1997).

In sum, in the UK and Europe, paradigmatic epidemiological studies aiming to investigate how social class (measured by employment status) impacts health played a key role in the development of Clusters 1 and 2. A shared focus on social inequalities in health outcomes facilitated links between these two clusters.

3.5.2. 'Inequalities' & 'disparities'

The lack of citation links between Cluster 3 and the European/UK clusters is a conspicuous feature of Fig. 1. US researchers in Fig. 1 seem to mostly cite other US researchers, whereas researchers in Australia, Europe and Canada seem more interconnected. This may be a reflection of the specialized streams of research apparent in Fig. 1, and specialized communities of (for example) geographers, economists, clinical epidemiologists or social-scientists, unaware of potentially relevant publications from other streams. Alternatively, these different terms may signify varied framings of health equity as biomedical or sociological phenomena. In interviews, most interviewees were unable to explain this feature of Fig. 1, and interviewees in both network hemispheres commonly indicated they had little sense of the other, underlining their separation:

> I have no idea who anyone is! I've never heard of any of those people! […]. How is this all getting done in silos? And Why??
>
> Social Epidemiologist, USA, Network Cluster 3.

> It is a bit hard to make sense of. There are a few names I've never heard of. I have to say […] I just can't think who these people are.
>
> Geographer, UK, Network Cluster 7.

Some interviewees suggested space between Cluster 3 (US dominated) and other clusters is due to a combination of terminological differences and contrasting research foci, with UK based researchers generally studying '*inequalities*' between social classes, while researchers in the US tend to study '*disparities*' between racial and ethnic groups (Kawachi et al., 2002). However, as we go on to elaborate, this was only a partial explanation.

The separation between 'inequalities' and 'disparities' scholars in Fig. 1 appears to reflect the distinct origins and independent development of two research traditions. Several inequalities scholars interviewed were keen to highlight the historical context of Cluster 1, in the wake of the Black Report (Black, Morris et al., 1980), the first systematic effort by any national government to understand and explain health inequalities between social classes (Smith, 2013). The 1980s, 1990s were periods of intense activity for British health inequalities scholars, as they attempted to address gaps in understanding identified by the Black Report, while documenting the health impact of policies put in place by the Thatcher-led Conservative government that had rejected the Report's conclusions (e.g. Whitehead, 1987). Many members of Cluster 1 and 4 pursued research on health inequalities throughout this period, despite limited funding, when the idea of health inequalities, even the term itself, was politically controversial:

> [It] was called the "health variations research program." We were told we couldn't use "inequalities" because Margaret Thatcher didn't like the term, so it was dumped and we were "variations".
>
> Cluster 1 Researcher, UK.

Interview data suggests this struggle contributed to a shared sense of identity among these scholars, now passed to some students and collaborators present in Clusters 1, 4 and 7. There was resistance toward adopting any term other than 'inequalities' among these UK interviewees, because the term had been fought for by researchers perceived as pioneers:

> Interviewer: Would you ever want to apply the term "Health Disparities" in the UK?
> I would rather we stuck with 'health inequalities'. […] During the Thatcher time, you weren't allowed to talk about health 'inequalities', I don't like dodging away from it.
>
> Cluster 1 Researcher, UK.

There was also a sense that some UK interviewees considered 'real' health inequalities research as being concerned exclusively with social determinants, and restricted to network Clusters 1, 4 and 7:

> There is that community, and there are factions within that community […] But we would all be seen as 'Health Inequalities' […] Some [are] more on the periphery, like [Researcher from Cluster 7] for example because [s/he's] more geography. [S/he] is more on the periphery, but a part of the family. And then there are almost like 'interlopers' of the mainstream […] they probably think that they're health inequalities researchers, but [they don't belong to] this group of people who *are* health inequalities, who have carried that trajectory within them, and have been shaped [by], and learned from, those pioneers.
>
> Cluster 4 Researcher, UK.

In contrast, in the US, the importance of social factors in determining health outcomes was catalysed by studies in the 1980s noting differences in medical practice across (apparently) similar patient populations (McPherson et al., 1982). Responding to this unexplained variation in medical care, the US government commissioned the "Health, United States, 1983" report, which described, for the first time, significant differences in "the burden of death and illness experienced by blacks and other minority Americans as compared with the nation's population as a whole" (p ix). The dominance of the term "health disparities" arose from this motivating drive to understand the 'gaps' in observed health outcomes and health care access between minority and majority ethnic populations (e.g. AMA CEDA, 1990).

This emphasis continued into the 21st century, with health disparities research in the US tending to focus on healthcare (*e.g.,* Fiscella et al., 2000; Nelson, 2002), while the European concern with inequalities in health relating to social class also persisted (Marmot et al., 2012). Several interviewees noted the longstanding divide between scholars studying health inequalities (in class) and health disparities (in race):

> Interviewer: Race is clearly very important to work on health disparities in the USA, it seems to be less of a focus in the UK?
> P: Yeah I've noticed that […] and I'm not sure why that is. [..] I haven't gone as deeply into it as I might, because it is not, to be quite blunt, it doesn't interest me that much, in the UK context.
>
> Cluster 4 Researcher, UK.

> Here [in the US] it is very much on race, we don't talk about class here. […] There is so much focus on race here. Some of it makes sense, and some of it is really misguided and misses the point.
>
> Cluster 3 Researcher, US.

US-based interviewees across the network, but most especially in Clusters 3 and 6, expressed concern regarding the way race is conceptualized in research, particularly within 'mainstream' epidemiology. NIH requirements to report findings by racial and ethnic subgroupings were positioned as contributing to the uncritical treatment of race in quantitative analyses:

> Important now is to talk about race and ethnicity, to talk about race being a sociological concept and not a biological reality. […] In the US, because many of us are getting federal funds, one has to design research that covers human variation, and that is designated as sex, race and now age. […] This notion that you have to design it into your study means then that you have to be able to assign a value […]. In assigning that value then you have pretty much said "this is an entity, these different racial categories really are entities."
>
> Cluster 6 Researcher, US.

In summary, in Fig. 1 we see the lasting impact of the way research about health equity was conceptualized and initiated on either side of the Atlantic. Insights from interview data reveal the ongoing importance of the way the two fields originated in the 1980s and developed over time, trajectories reflected in the structure of Fig. 1. Perhaps more importantly, it is clear that 'inequalities' and 'disparities' are, in practice, not interchangeable terms for the same phenomena. To use one term aligns a project with a particular tradition of research, a group of pioneering investigators, and a historical conceptualisation of equity. Overlying geographic variation was disciplinary variation in use of these terms, discussed in the next section.

3.5.3. *Disciplinary diversity*

The distribution of doctoral trainings is not uniform across the network, mostly due to the mix of humanities, political-, life- and social-sciences on the network's left side (which, as Fig. 2 illustrates, tend to study 'health inequalities'), and the dominance of medical, statistical, health promotion and epidemiological backgrounds on the right side (where 'health disparities' dominates as the preferred term). Researchers with economic training are similarly concentrated in Cluster 2, as are the majority of geographers (Cluster 7). The small, US dominated clusters (5 and 8) contain many more cancer epidemiologists and health promotion scholars than the wider network, and represent a small number of institutions. Clusters 7 and 8 have the most recent median first publication date, and appear to represent regional communities of disciplinary and topical specialists. Clusters 1 and 3 (foundational clusters within the UK and US, respectively) have the same SNE diversity, though Cluster 1 might be considered more diverse as it is smaller.

One notable anomaly is Cluster 6, which includes a mix of 'health inequalities' and 'health disparities' researchers, appears on the health inequalities 'side' of Fig. 1 but does not include any members from the UK. Many members of Cluster 6 have social science backgrounds, and interviewees from this cluster explicitly framed the drivers of health inequity as socially-situated, which may explain the location of this group in citation space alongside clusters with strong social science membership. Nevertheless, interviewees in Clusters 1, 4 and 7 were almost universally unaware of the work proceeding in Cluster 6.

3.5.4. *Disease focus*

In addition to varied use of terminology, and different disciplinary profiles, the two network hemispheres differ in their disease foci, reflected in the algorithmic detection of two cancer-specific, US-dominated citation clusters (5 and 8) on the network's right side. This difference may be at least partially due to the data availability landscape within the US throughout the 1990s. In 2002, six members of Cluster 3 expressed their frustration that "few or no socioeconomic data exist in most US public health surveillance databases" (Krieger et al., 2002). In the context of this scarcity, the Surveillance, Epidemiology and End Results registry (SEER) held high-quality cancer incidence and outcome data alongside patient demographics dating back to 1973 (National Cancer Institute), representing a crucial data source for health disparities scholars. Between 2000 and 2010, several members of Clusters 3, 5 and 8 utilised SEER data to demonstrate racial disparities in cancer screening, incidence, and health outcomes (*e.g.,* Singh et al., 2004). Analysis of SEER data to investigate disparities in cancer-related outcomes was also utilised to demonstrate best-practice methodology (Harper et al., 2008). Cancer disparities research continues to be well-funded in the US, and is supported institutionally via Comprehensive Cancer Centers. Many interviewees in the US suggested that the security of cancer-specific funding powerfully shapes research about health disparities:

> Interviewer: Many US researchers who have appeared in my bibliometric network study cancer. Why do you think that is?
> Because it's sexy. And well-funded.
>
> Cluster 3 Researcher, US.

For interviewees in Clusters 3 and 8, there was a sense that studying cancer is a financial necessity, and that funding streams shape research questions:

> We're pushed and funded to examine disparities in single disease conditions. And I am very guilty of this […] There are common underpinnings across a number of health behaviours, health conditions, health outcomes […] but in the US it is very disease focused, that is how our funding streams are organised. Cluster 8 Researcher, US.

Interviewees from the UK also discussed the epistemic impact of research funding:

> We just ignore the 90% of what causes inequalities and focus on the one bit where we can get money and we can have control. I can see how that actually impacts on me […] even though you know, fundamentally, it will only make a tiny bit of difference.
>
> Cluster 4 Researcher, UK.

Partly as a reaction to the dominance of medical and disease-specific models in the US (Honjo, 2004), some US-based epidemiologists have advanced Social Epidemiology: "the study of how the social world influences–and in many cases defines–the fundamental determinants of health" (Berkman et al., 2014). These researchers, located in Cluster 3, helped cement social epidemiology as a legitimate sub-disciplinary specialisation (Galea and Link, 2013). Popular textbooks and key theoretical contributions to the field have been authored by members of Cluster 3, and the journal *Social Science and Medicine* (where many such contributions are published), is edited by members of Cluster 3. As these social epidemiologists are advancing a view of health as socially (rather than biomedically) situated, it is unsurprising that these scholars are located in the region of Cluster 3 closest to the social-science dominated network clusters.

4. Concluding discussion

Our bibliometric analysis confirms and for the first time visualizes researchers' accounts of health equity research as a field comprised of clusters which are generally not well-connected. Our researcher profile and interview data suggest that, although disciplinary training played a role in the emergence of these clusters, so too have historical, geographic, institutional and financial (research funding) forces. Interpreting these results through the lens of Science and Technology Studies theorists Sheila Jasanoff and Thomas Kuhn helps to contextualise results within existing theoretical pictures of scientific progress.

4.1. *A cartographic model of multi-discipline research*

Jasanoff (2012) described two cartographic models of multidisciplinary research. The first, as nations within a continent, uninterrupted territory divided among disciplinary 'states', without space in between. The second, as an archipelago: wherein "the disciplines are oddly and idiosyncratically bounded formations, haphazardly scattered across a sea of ignorance, with unexplored waters in between." (p192). In the continental model, disciplinary boundaries are broken by the development of *interstate highways*, in the archipelago model, by *voyages into the unknown*. Health equity research, as visualised in Figs. 1 and 2, appears to display characteristics of both models, as it presents a central land-mass without gaps (Clusters 1, 4, 7 and 3), as well as peninsulas (Clusters 6 and 2), and islands (Clusters 5 and 8).

Social epidemiology might be described as a 'highway' between the biomedically-dominated, right-hand-side of the network and the social-science-rich left hand side. The appearance of social epidemiology as a distinct paradigm within mainstream epidemiology appears to be holding the two research communities together in citation-space, as this interdisciplinary specialty supports the interweaving of diverse perspectives. However, the 'bridge' researchers in Cluster 3 do not have strong links to the critical, policy-focused strand of health inequalities research produced by authors in Cluster 4, pointing to an apparent gap between US-led research on social epidemiology and critical policy analysis.

Collaboration on large-scale projects, and shared historical framings of equity pave the intellectual carriageways linking clusters 1, 2, 4 and 7. A shared conceptualisation of health as socially-situated links Clusters 6, 4 and 1. Cluster 2 is a well-connected methodological hub, and its central position reflects the high value placed on quantitative methodology across the field.

The 'unexplored waters' of health equity scholarship appear to lie in the gulf between (mainly) European scholars of the relationship between health and social class and the (mainly) US scholars of the intersection between health and race/ethnicity. Despite strong links to the epidemiologically-driven Cluster 1, Cluster 3 is poorly connected to the social science-dominated clusters 6, 7 and 4. We have shown that linguistic differences ('inequalities' vs 'disparities') do not fully explain this lack of connectivity, and are rather themselves a reflection of distinct, mature research traditions, each with their own history, disciplinary character, and funding landscape.

4.2. The long shadow of exemplary past achievement and the centrality of research funding

Although our findings make clear that disciplinary training alone cannot explain the clusters comprising health equity research, disciplinary siloing is clear. Thomas Kuhn's presentation of science as proceeding within independent paradigmatic communities (1962) highlights the ways in which early scholarly achievement powerfully shapes the trajectory of enquiry within disciplines. Kuhn's sense of disciplinary paradigms as "exemplary past achievement" (postscript to Kuhn, 1970) seems relevant, as early framings of health equity in the 1980s have cast long temporal shadows across Fig. 1. These early framings also appear to have influenced the attraction and recruitment of disciplinary specialists to the field; in the UK, where health inequalities research was politically controversial, sociology and political science are well-represented, and now present established, independent traditions of health equity scholarship. In the US, where health disparities began with unexplained variation in clinical practice, the clinical disciplines such as medicine, nursing, clinical epidemiology and psychology are more dominant, and 'cancer disparities' has emerged as a free-standing research domain, partly in response to relevant independent funding streams. The Kuhnian view warns against the prospect that these differences might be bridged simply, via multi-disciplinary teams, given the challenges of working across disciplines with fundamentally different paradigmatic assumptions. Indeed, future research might explore how researchers' ontological or epistemological outlooks impact on collaboration within health equity research, and public health more broadly.

Cutting across these historical, geographical and disciplinary norms are challenges relating to research funding. Our analysis highlights the importance of funding in shaping research content, and illustrates how 21st Century work reflects path-dependencies established many decades prior, in response to data access challenges and/or the design of foundational studies. Reflecting previous findings (Garthwaite et al., 2016; Smith, 2010), interview data illustrate that health equity researchers consider what is likely to be funded when making decisions about the direction of their work, and this appears to pull researchers towards disease-specific questions. Overall, we suggest that the formation of insular scholarly sub-networks within health equity research may be a consequence of the ways in which broad, motivating questions are rendered both scientifically manageable, and fundable.

Returning to the critiques outlined in our Introduction, an optimistic account might suggest that, having provided a visual and descriptive map of the field it will be possible for researchers to enhance cross-cluster dialogue, by engaging with work in clusters with which they are unfamiliar. It might also be feasible for interested funders to further incentivise the development of such interdisciplinary links. However, our analysis highlights the ways in which the complex nexus of factors underpinning the field's structure may also condition the field's content. With so many intersecting drivers, overcoming fragmentation means more than doing different work, it means being open and supported (institutionally and financially) to do work differently. The combined influences of funding, data availability, disciplinary norms and preferences, and national and institutional research cultures may render this flexibility difficult to achieve, in practice.

Likewise, interpreting the stable terminological division between 'health inequalities' and 'health disparities' research with an awareness of the performative nature of language in academia suggests this divide may be hard to bridge (i.e. use of particular terms to signal familiarity with, and to position new work in relation to what has gone before, and/or to demonstrate authors' normative values). Finally, as one reviewer reflected, competition over research funding can lead to researchers trying to protect their 'territory', as one interviewee's reference to 'interlopers' implied (see also Gieryn, 1983). Relatedly, these distinct territories of expertise may be serving distinct 'clients'. Yet, without greater cohesiveness, lessons from policy studies suggests efforts to effect policy change to reduce health differences are likely to struggle (Smith, 2013).

At the time of writing, the unequal burden of COVID-19 across social groups is intensifying scholarly and public interest in health equity. In response to the pandemic, the need for studies focused on racial inequity in health in the US and UK has been highlighted (Bhala et al., 2020), suggesting that COVID-19 may represent a force capable of more closely uniting 'health inequalities' and 'health disparities' scholarship. However, while this study supplies an atlas for scholars to move beyond their existing networks, step outside the template of past successes, forge new alliances and explore less well-trodden paths, we have also detailed several reasons why researchers may be reluctant or unable to take these steps. Future studies might aid such efforts further, by examining the factors underlying this field's fragmentation in detail, including resources, the role of ontology, epistemology, disciplinary training and normative-political values in shaping researchers' perceptions of what is possible, what is feasible, and what is necessary to reduce observed inequity in health.

Contribution statement

TC conceived and designed the study, undertook all data collection and analysis, and produced the first draft of the article. KS provided advice on the study design, data collection and analysis and contributed to drafting the final manuscript, notably the abstract, introduction and concluding discussion.

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References

AMA Council on Ethical and Judicial Affairs (CEDA), 1990. Black-white disparities in health care. J. Am. Med. Assoc.: JAMA, J. Am. Med. Assoc. 263 (17), 2344–2346. https://doi.org/10.1001/jama.1990.03440170066038.

Bambra, C., Smith, K.E., Garthwaite, K., Joyce, K.E., Hunter, D.J., 2011. A labour of sisyphus? Public policy and health inequalities research from the black and acheson reports to the Marmot review. J. Epidemiol. Comm. Health 65 (5), 399–406. https://doi.org/10.1136/jech.2010.111195.

Bartley, M., 1992. Authorities and Partisans: the Debate on Unemployment and Health. Columbia University Press.

Bhala, N., Curry, G., Martineau, A.R., Agyemang, C., Bhopal, R., 2020. Sharpening the global focus on ethnicity and race in the time of COVID-19. Lancet 395 (10238), 1673–1676. https://doi.org/10.1016/S0140-6736(20)31102-8.

Berkman, L.F., Ichiro, K., Glymour, M.M. (Eds.), 2014. Social Epidemiology. Oxford University Press. https://oxfordmedicine.com/view/10.1093/med/9780195377903.001.0001/med-9780195377903.

Black, D., Morris, J.N., et al., 1980. Inequalities in Health: Report of a Research Working Group. Department of Health and Social Services, London.

Bouchard, L., Albertini, M., Batista, R., de Montigny, J., 2015. Research on health inequalities: a bibliometric analysis (1966–2014). Soc. Sci. Med. 141, 100–108. https://doi.org/10.1016/j.socscimed.2015.07.022.

Braveman, P., 2006. Health disparities and health equity: concepts and measurement. Annu. Rev. Publ. Health 27 (1), 167–194. https://doi.org/10.1146/annurev.publhealth.27.021405.102103.

Cash-Gibson, L., Rojas-Gualdrón, D.F., Pericàs, J.M., Benach, J., 2018. Inequalities in global health inequalities research: a 50-year bibliometric analysis (1966-2015). PloS One 13 (1), e0191901. https://doi.org/10.1371/journal.pone.0191901.

Fiscella, K., Franks, P., Gold, M.R., Clancy, C.M., 2000. Inequality in quality: addressing socioeconomic, racial, and ethnic disparities in health care. J. Am. Med. Assoc. 283 (19), 2579–2584. https://doi.org/10.1001/jama.283.19.2579.

Galea, S., Link, B.G., 2013. Six paths for the future of social epidemiology. Am. J. Epidemiol. 178 (6), 843–849. https://doi.org/10.1093/aje/kwt148.

Gamble, V.N., Stone, D., 2006. U.S. Policy on health inequities: the interplay of politics and research. J. Health Polit. Pol. Law 31 (1), 93–126. https://doi.org/10.1215/03616878-31-1-93.

Garthwaite, K., Smith, K.E., Bambra, C., Pearce, J., 2016. Desperately seeking reductions in health inequalities: perspectives of UK researchers on past, present and future directions in health inequalities research. Sociol. Health Illness 38 (3), 459–478. https://doi.org/10.1111/1467-9566.12374.

Gieryn, T.F., 1983. Boundary-work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists. Am. Socio. Rev. 48 (6), 781–795. https://doi.org/10.2307/2095325.

Graham, H., 2004. Social determinants and their unequal distribution: clarifying policy understandings. Milbank Q. 82 (1), 101–124. https://doi.org/10.1111/j.0887-378X.2004.00303.x.

Harper, S., Lynch, J., Meersman, S.C., Breen, N., Davis, W.W., Reichman, M.E., 2008. An overview of methods for monitoring social disparities in cancer with an example using trends in lung cancer incidence by area-socioeconomic position and race-ethnicity,1992–2004. Am. J. Epidemiol. 167 (8), 889–899. https://doi.org/10.1093/aje/kwn016.

Honjo, K., 2004. Social epidemiology: definition, history, and research examples. Environ. Health Prev. Med. 9 (5), 193–199. https://doi.org/10.1007/BF02898100.

Huang, Ronggui, 2016. RQDA: R-based qualitative data analysis. R package version 0.2-8. http://rqda.r-forge.r-project.org/.

Jasanoff, S., 2012. A field of its own: the emergence of science and technology studies. In: Frodeman, R., Klein, J.T., Mitcham, C. (Eds.), The Oxford Handbook of Interdisciplinarity, second ed. Oxford University Press.

Jost, L., 2007. Partitioning diversity into independent alpha and beta components. Ecology 88 (10), 2427–2439. https://doi.org/10.1890/06-1736.1.

Kakwani, N., Wagstaff, A., van Doorslaer, E., 1997. Socioeconomic inequalities in health: measurement, computation, and statistical inference. J. Econom. 77 (1), 87–103. https://doi.org/10.1016/S0304-4076(96)01807-6.

Kawachi, I., Subramanian, S.V., Almeida-Filho, N., 2002. A glossary for health inequalities. J. Epidemiol. Comm. Health 56 (9), 647–652. https://doi.org/10.1136/jech.56.9.647.

Kawachi, I., Kennedy, B.P., 1997. The relationship of income inequality to mortality: does the choice of indicator matter? Soc. Sci. Med. 45 (7), 1121–1127. https://doi.org/10.1016/S0277-9536(97)00044-0.

Krieger, N., Chen, J.T., Waterman, P.D., Soobader, M.J., Subramanian, S.V., Carson, R., 2002. Geocoding and monitoring of US socioeconomic inequalities in mortality and cancer incidence: does the choice of area-based measure and geographic level matter? Am. J. Epidemiol. 156 (5), 471–482. https://doi.org/10.1093/aje/kwf068.

Kuhn, T.S., 1962. The Structure of Scientific Revolutions. University of Chicago Press.

Kuhn, T.S., 1970. The Structure of Scientific Revolutions, second ed. University of Chicago Press.

Lynch, J., 2017. Reframing inequality? The health inequalities turn as a dangerous frame shift. J. Publ. Health 39 (4), 653–660. https://doi.org/10.1093/pubmed/fdw140.

Mackenbach, J.P., van de Mheen, H., Stronks, K., 1994. A prospective cohort study investigating the explanation of socio-economic inequalities in health in The Netherlands. Soc. Sci. Med. 38 (2), 299–308, 1982.

Mackenbach, Johan P., Valverde, J.R., Artnik, B., Bopp, M., Brønnum-Hansen, H., et al., 2018. Trends in health inequalities in 27 European countries. Proc. Natl. Acad. Sci. Unit. States Am. 115 (25), 6440–6445. https://doi.org/10.1073/pnas.1800028115.

Manor, O., Matthews, S., Power, C., 1997. Comparing measures of health inequality. Soc. Sci. Med. 45 (5), 761–771. https://doi.org/10.1016/S0277-9536(96)00412-1.

Marmot, M.G., Allen, J., Bell, R., Bloomer, E., Goldblatt, P., others, 2012. WHO European review of social determinants of health and the health divide. Lancet 380 (9846), 1011–1029.

McCartney, G., Collins, C., Mackenzie, M., 2013. What (or who) causes health inequalities: theories, evidence and implications? Health Pol. 113 (3), 221–227. https://doi.org/10.1016/j.healthpol.2013.05.021.

McPherson, K., Wennberg, J.E., Hovind, O.B., Clifford, P., 1982. Small-area variations in the use of common surgical procedures: an international comparison of New England, England, and Norway. N. Engl. J. Med. 307 (21), 1310–1314. https://doi.org/10.1056/NEJM198211183072104.

Mongeon, P., Paul-Hus, A., 2016. The journal coverage of Web of Science and Scopus: a comparative analysis. Scientometrics 106 (1), 213–228. https://doi.org/10.1007/s11192-015-1765-5.

National Cancer Institute, 2019. SEER registries—about SEER. SEER. https://seer.cancer.gov/registries/index.html. (Accessed 27 July 2019).

Nelson, A., 2002. Unequal treatment: confronting racial and ethnic disparities in health care. J. Natl. Med. Assoc. 94 (8), 666–668.

Pool, R., Ning, H., Lloyd-Jones, D.M., Allen, N.B., 2017. Trends in racial/ethnic disparities in cardiovascular health among US adults from 1999–2012. J. Am. Heart Assoc. 6 (9), e006027 https://doi.org/10.1161/JAHA.117.006027.

Porter, A.L., Rafols, I., 2009. Is science becoming more interdisciplinary? Measuring and mapping six research fields over time. Scientometrics 81 (3), 719. https://doi.org/10.1007/s11192-008-2197-2.

Qureshi, K., 2013. It's not just pills and potions? Depoliticising health inequalities policy in England. Anthropol. Med. 20 (1), 1–12. https://doi.org/10.1080/13648470.2012.747593.

Rafols, I., Meyer, M., 2007. How cross-disciplinary is bionanotechnology? Explorations in the specialty of molecular motors. Scientometrics 70 (3), 633–650. https://doi.org/10.1007/s11192-007-0305-3.

Rose, G., Marmot, M.G., 1981. Social class and coronary heart disease. Heart 45 (1), 13–19. https://doi.org/10.1136/hrt.45.1.13.

Singh, G.K., Miller, B.A., Hankey, B.F., Edwards, B.K., 2004. Persistent area socioeconomic disparities in U.S. incidence of cervical cancer, mortality, stage, and survival. Cancer 101 (5), 1051–1057. https://doi.org/10.1002/cncr.20467, 1975–2000.

Slattery, M.L., Lundgreen, A., Torres-Mejia, G., Wolff, R.K., Hines, L., Baumgartner, K., John, E.M., 2014. Diet and lifestyle factors modify immune/inflammation response genes to alter breast cancer risk and prognosis: the Breast Cancer Health Disparities Study. Mutat. Res. 770, 19–28. https://doi.org/10.1016/j.mrfmmm.2014.08.009.

Smith, K., 2013. Beyond Evidence Based Policy in Public Health: the Interplay of Ideas. Springer.

Smith, K.E., Eltanani, M.K., 2014. What kinds of policies to reduce health inequalities in the UK do researchers support? J. Publ. Health. https://doi.org/10.1093/pubmed/fdu057.

Smith, K., 2010. Research, policy and funding – academic treadmills and the squeeze on intellectual spaces. Br. J. Sociol. 61 (1), 176–195. https://doi.org/10.1111/j.1468-4446.2009.01307.x.

Smith, K.E., Bambra, C., Hill, S.E. (Eds.), 2015. Health Inequalities: Critical Perspectives. Oxford University Press.

Statacorp. *Stata Statistical Software: Release* vol. 16.College Station, TX: StataCorpLLC.

Van Eck, N.J., Waltman, L., 2017. Citation-based clustering of publications using CitNetExplorer and VOSviewer. Scientometrics 111 (2), 1053–1070. https://doi.org/10.1007/s11192-017-2300-7.

Wade, B.H., Stone, J.H., 2010. Overcoming disciplinary and institutional barriers: an interdisciplinary course in economic and sociological perspectives on health issues. J. Econ. Educ. 41 (1), 71–84. https://doi.org/10.1080/00220480903382198.

Wagstaff, A., Culyer, A.J., 2012. Four decades of health economics through a bibliometric lens. J. Health Econ. 31 (2), 406–439. https://doi.org/10.1016/j.jhealeco.2012.03.002.

Waltman, L., Van Eck, N.J., 2013. A smart local moving algorithm for large-scale modularity-based community detection. Eur. Phys. J. B 86 (11), 471. https://doi.org/10.1140/epjb/e2013-40829-0.

Ward, A., Johnson, P.J., O'Brien, M., 2013. The normative dimensions of health disparities. J. Health Dispar. Res. Prac. 6 (1). https://digitalscholarship.unlv.edu/jhdrp/vol6/iss1/4.

Whitehead, Margaret, 1987. The Health Divide: Inequalities in Health in the 1980s. Health Education Council.

Appendix I: The Spectrum of Statistical Literacy

Interviewees had varying confidence in their statistical skills, which in many cases seemed inconsistent with my impression of their experience and expertise. Some researchers who declared themselves ignorant or unsophisticated went on to discuss statistical methods with some nuance and with a critical perspective. I detected a generalised anxiety around statistics for many interviewees, which some senior academics reported managing within their research teams:

> TC: Perhaps we can chat a bit about statistics. […]
>
> I'm not intimidated by statistics
>
> TC: Have you experienced that other people are?
>
> Yeah. People perform it.[…] You meet people who do that thing, where **you say 'you might need to learn some stats' and they sort of start to crumple up a bit.** You also meet people at the other end of the spectrum […] essentially, 'if you don't understand this massively complex algebraic statement then **you are inferior'**.
>
> Public Health researcher (Geography PhD)

Statistical proficiency seems to be surrounded by intense emotions (e.g., fear) and low statistical literacy was repeatedly connected to feelings of inferiority and inadequacy, especially by interviewees with social science backgrounds in Clusters 4 and 6.

> Statistics has this power […] It relates to the whole discussion we are having, including the search for legitimacy.
>
> Public Health researcher & Medical Doctor (Social Science PhD)

One interviewee did enthusiastically claim the title of 'statistical expert' for themselves. However, my impression was that this interviewee had not much more understanding than an undergraduate class in regression methods would provide. Therefore, an important quality of statistical expertise in the context of research about health is that it is *relative*. An individuals' status as expert or non-expert depends on context. One interviewee who had moved across disciplines (and did not consider themselves expert) reflected that being familiar and confident with basic regression methods *did* confer expert status in some scientific contexts, but was viewed as grossly inadequate in others:

> *I remember thinking* ***"they're all statisticians or demographers*** *in that team, I will never understand this stuff.* ***I'll just never believe in myself sufficiently"****. Whereas, working with the social policy people, my open university statistics was absolutely brilliant! [Laughing]*

Medical Sociologist, Social Policy PhD

The identity of ‘statistical expert’ therefore takes different forms in varied settings. Despite this variation, the value and importance of statistical literacy was acknowledged by almost all interviewees, but most especially interviewees in clusters 3 and 8 (highlighted in figure RC5.1 below), who presented statistical capability as being key for maximising their research output, obtaining grant funding, and maximising individual employability:

> Ramp[ing] up your quantitative skills is never going to be a disservice. […] One thing I feel allows me to do things quickly, and to accelerate my ability to disseminate work is being comfortable quantitatively.

Health Behaviour Researcher (Health Behaviour PhD)

> In biostatistics, after 20 years, still the job market is number one - it is so good!
>
> Biostatistician B (Biostatistics PhD)

The status of statistical professionals was also noted to have increased over recent decades.

Decaying statistical proficiency across the career

Despite being a valued skill, several senior academics reported declining statistical literacy across their careers. Eight interviewees from across the network specifically mentioned that while early in their careers they conducted analyses themselves, they no longer do so:

> I’m not a statistician, I used to be more adept when I was younger, now I have people doing it for me.
>
> Epidemiologist A (Epidemiology PhD)

> My first papers I wrote […] I did my own statistics, which is scary as all get-out. I’m not allowed to do that anymore, we have statisticians here who run all that stuff.
>
> Occupational health researcher (Anthropology PhD)

Three of these researchers described their current position as one of ‘dependency’:

> I’m totally dependent on my statistician friends.
>
> Epidemiologist (Anthropology PhD)

Tellingly, no interviewee in Cluster 2 discussed losing touch with their statistical ability over time, or described themselves as being dependent on statisticians. This is perhaps unsurprising, as, recalling the findings of RC1, this cluster is notable for containing a number of experts in inequality *measurement*. Interviewees in and near Cluster 2 were the most critical of the statistical practice they observed in the literature, and at conferences. In this part of the network - dominated by economists and epidemiologists - statistical literacy is essential, statistical ability appears to remain important across a career, and potential collaborators are evaluated based on (among other things) their quantitative skills.

If statistical literacy carries connotations of legitimacy, productivity and employability, and statisticians are the only ones 'allowed' to touch research data in some settings, statistical expertise appears to be accompanied by a kind of power. In some settings, especially medical schools in the UK and Cancer Centres in the USA, interviewees reported that statisticians are the *only* researchers who generate analytical products from raw quantitative data. If statistical skill is so highly valued, it therefore seems curious that researchers step back from quantitative analysis as they advance in their careers.

For researchers lacking confidence with statistical analysis, engaging a specialist to perform their analyses was a welcome chance to delegate a confusing and stressful element of research conduct. These researchers reported limping through early publications without deep understanding, and joyously outsourced statistical work as soon as resources permitted. But researchers *with confidence* in their abilities also reported doing less hands-on analysis as they moved up the ranks, sometimes with regret:

> So, I used to do my own analysis […] now I usually hand it over to people […] I don't literally do the programming anymore, which I'm a little sad about.
>
> Health Equity Researcher (Nursing PhD)

These interviewees reported that statistical analysis takes too long and gets in the way of other tasks - most especially tasks which generate research funding. The point of transition from managing individual analyses and papers to driving a funded research program (writing competitive grants and guiding/developing research questions pursued by others) seems to be the point at which many researchers stepped back from conducting statistical analyses themselves:

TC: Do you do your own statistical analysis?

Right now [I do]. I am hoping to transition out of doing that, because it takes forever and I am at a point where I need to write a bunch of grants.

Social Epidemiologist A (Epidemiology PhD)

Doing ones own statistical analysis was presented in tension with ‘efficient’ research output by interviewees in the UK, Australia, and the USA:

> So I used to do my own analysis, and then as I got busier and busier I was less efficient at it.
>
> Health Equity Researcher (Nursing PhD)

Here, then, are competing incentives for some researchers who study health equity. There is a widespread perception that quantitative skills are valuable, even essential, but for some interviewees they are not as valuable as the efficient generation of grant applications and peer-reviewed publications. Outside of Cluster 2 there was a general sense that researchers should abandon developing the former in pursuit of the latter as soon as practicable:

> I used to do all my own stuff […] I’m always impressed, when people like [blank] used to do a lot of the stats herself, I think she really enjoyed doing it, and at one level I was kind of impressed, but on the other level I was thinking “This is the most inefficient thing. You could be doing five things if you had a team, and you cost a lot.”
>
> Public Health researcher, Social Science PhD

One interviewee who conducts qualitative research noted that there are very few opportunities to develop quantitative skills in the mid or late career. In a phrase reminiscent of the interviewees quoted in RC2 who felt that the need to obtain funding acted as a ‘hamster wheel’, restricting the questions they could pursue, several researchers described being on a methodological ‘track’ from which they found it difficult to deviate:

> Funders are investing in training people at an early career stage, it is not something funders are investing in the mid career or late career stage […] that would require an academic context in which there is more space. Because if you do that training properly you are taking time out from your teaching and publishing, and so on.
>
> Health Policy Researcher (Geography PhD)

This very brief analysis contains two findings of potential significance within public health. First, the view of statistical expertise as varying in varied contexts aligns with existing studies of scientific expertise, and Jasanoff (2003) stresses that expertise

> is not merely something that is in the heads and hands of skilled persons, constituted through their deep familiarity with the problem in question, but rather that **it is something acquired, and deployed,**

within particular historical, political, and cultural contexts. (Jasanoff, 2003, p. 393)

Debates in epidemiology and public health tend to treat ‘statistical literacy’ as something researchers either possess or lack, however, this brief analysis of statistical expertise in HIDR suggests a more nuanced approach may be required.

Secondly, hyper-prolific authors in science have been identified as a topic of concern in health-related research. Thousands of authors demonstrating ‘implausible’ rates of publication were identified by Ioannidis et al (2018) and, excluding large physics collaborations, around half of these authors publish in health-related fields. A survey of these hyper-prolific authors revealed that 10% of respondents read fewer than 25% of papers on which they are named as authors, and data from this PhD project suggests that statistical literacy and hands-on contact with data may be casualties of the drive - within some disciplines - to publish very high volumes of papers. As senior, successful authors delegating statistical work become role-models for young scientists, statistical expertise may likewise be viewed as an optional or unimportant part of a successful research career in health-related research. A trend away from hands-on contact with data may be concerning to those wishing to promote statistical literacy within epidemiology and public health, and future studies might explore the mechanics of this trend in greater detail.